

From  
The Autobiography of  
Economic Theory  
AND OTHER REFLECTIONS

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KALYANI PUBLISHERS  
NEW DELHI—LUDHIANA

# KALYANI PUBLISHERS

H. O. :—1/1 Rajinder Nager, Ludhiana

B. O. :—23, Daryaganj, New Delhi

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PRINTED IN INDIA

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PUBLISHED BY USHA RAJRUMAR FOR KALYANI PUBLISHERS  
AND PRINTED IN MODERN PRINTERS, NAVIN SHAHDARA  
DELHI—110032.

To Suda, my wife





## PREFACE

Much of what follows is part of what I have been teaching my post-graduate students over the last two decades or so. Proof enough of how a teacher can confuse as well, of course, as be confused. I do, at all events, hope that the reader will not be misled by the rather conversational, even after-dinner-speech, style: it's all (serious) economics—or, at least, meant to be. A more or less proper textbook version, which will include quite a bit more, might come out under the title, *The Behaviour of Product*.

Vikas Mishra

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## CHAPTER I

### From the Autobiography of Economic Theory\*

I might begin by confessing to a certain feeling of superiority, something (even if it were judged by some of my sister-theories as a complex) for which, as I reckon, I cannot blame myself. In a creation where there are low as well as high-brow theories, it is, given my awareness of my undisputed worldly-wise prosperity (which even the highest of high-brows cannot help, amusedly or resignedly, envying), only natural that, in my assessment of my intellectual predisposition, I should be contemplating mainly those that are, self-consciously, low-brow. It is universally recognised that I am not a low-brow, and it is, after all, near-vulgar to begin to quarrel about how high-brow I was. Of course, I am at times conscious of the possibility that some theories might be, or might be considering themselves to be, higher-brow than me. But, then, I am, in such depressing moments, almost always, almost wholly, rescued by my suspicion that such theories could not possibly be knowing about me well enough to be able or willing to stick their necks out far enough convincingly to begin to take public position about any of my supposed (theoretic) deficiencies. The position about my own reflections about myself, however, is a little short of being entirely satisfactory, especially, I might own, as I cannot escape a vague realization that there is perhaps something fishy about me. I cannot, indeed, ignore the gnawing impression that my feeling of superiority might, after all, be emanating from a certain, less obvious, feeling of inferiority. My autobiography itself owes its beginnings to the urge to probe into myself as objectively as I 'theoretically' (in the sense in which human beings say 'humanly') can.

My origins, if, as is my wont, I abstract, in this case from historical time, do not present a problem, and are easily conceptualized. As is the case with all us theories, I was born when certain

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\*Published in *Business, Economics & Planning* (Essays in honour of Professor A.B. Ghosh). Edited by P.K. Ghosh, V.S. Minocha and D.B. Gupta.

questions and their answers, as pertained what I was supposed to be about, emerged as a separate, identifiable, group from out of the more or less undifferentiated and certainly rather inelegant something called knowledge. It was (it always is in our case) severance of a double umbilical chord, as it were: I became different quantitatively as a separate entity—and qualitatively as a systematized body (of knowledge). Systematized, Oh, I have said it; it is this that, whenever I think of it, haunts me, follows me as one's shadow does, frustrates me—it is killing. But I am anticipating.

And I must distinguish myself from others of my species. But, first, the similarities. We all appear from the need to explain, and, despite the distinction between ourselves and our, let me call, prototypes, hypotheses, which when verified become us, we are all, basically, provisional in that we can be replaced by newer and more powerful us. Just as inference is the expression of relation of implication between propositions, something that is the sole preoccupation of the formal sciences (formal logic and pure mathematics), we belonging to the empirical sciences simply represent and express the inter-connections, as they obtain, among facts, in the world of reality. We are 'good' or 'bad' according as we do or do not truthfully represent the inter-connections; the nearer we are to being true—materially true—the more powerful we are as, which is what we are, explanatory constructs. The inter-connections themselves, which we express as a set of related propositions, relate the fact (behaviour) to be explained to the fact or facts that are found to be so connected with the former as to be treated as the explanatory variables. Of course, there are, in economics as in the other sciences, as many theories as the empirical inter-connections discovered, which, since there could be alternate, competing, theories, may be more than the number of problems raised, behaviour to be explained. Naturally, my using the first person singular is a manner of speaking, apart from the fact that, being a separate, identifiable, entity, economics, like the other sciences, exhibits a certain affinity among its various theories. Since the facts a science deals with, the so-called subject matter, must needs have an affinity (that's what makes one science different from another), the theories in a science, too, must necessarily imbibe a certain affinity among themselves. And now I cannot avoid feeling the frustrating shadow following me.

The fact is that the extent, if not also the nature, of affinity among theories is not the same in the different sciences. In the physical sciences, for instance, the explanatory system (the body of inter-related theories) exhausts as well as it is exhausted by the range

of behaviour that calls for explanation. In other words, any sharp distinction there between the explanatory variables and the variable to be explained is little more than expository since the elements in both categories, in a reversible functional relationship, as it were, get explained by one another all at once. In this sense, the physical sciences may be said to be more or less self-contained or, to put it differently, the formulation of the theoretical system there is truly circular—almost wholly systematized. In the biological and, most pronouncedly, social sciences, on the other hand, the explanatory variables are 'external' as well as 'internal', as it were; and the only way to internalize them all (to look for the explanation of the external explanatory variables, and for that of the explanatory variables of the preceding set of explanatory variables, and so on) is to exhaust the universe, an absurdity. But, of course, the consequence is that the formulation of the theoretical system in such cases is far from being truly circular, systematized.

Not, probably, as well as those in the physical sciences, I manage to do, even in this respect, well enough when compared with my sister-theories in the other social sciences, other than economics, that is; in fact, I leave them all behind, I think, by a long pole. Even when compared with the natural sciences, I sometimes feel less than usually pessimistic. And not because I have in mind some such thing as the Walrasian general equilibrium system, with its something like reversible functional relationships and all that. I dare say I am not terribly proud of it. It is imbecile in a crucial respect. It cannot possibly illuminate, even grasp, the picture as relates to the aggregate of quantities, the national product. And, of course, it's not me, not a theory. Naturally, it has me (a theory) behind it, but what that me is I shall have occasion, unfortunately, to speak about a little later on. No, I have something less exciting in mind. I think I can say that, irrespective of the degree of systematization, the theories in the empirical sciences (unlike formal logic and pure mathematics where the term of reference is validity of reasoning and where, evidence being conclusive, one has conclusions which are necessary) cannot (evidence for material truth, for truth about the empirical world, being always partial) expect to muster conclusive evidence; consequently, the conclusions we carry are probabilistic, not necessary. The position, in principle, is the same for us all; for the highest of high-brow of the physical as well as for the lowest of low-brow of the social sciences. I realise that probability can be as high as one and as low as zero, and my depression threatens to return. The physical sciences, moreover, have the advantage as well of unquestionable

quantification, precise measurement—and also of experimentation; and while the method of science, the method which helps sift evidence, may be basically the same for all the sciences, I cannot assert that the aforesaid advantages do not tend to make some theories more powerful than others. Some among us, alas, are more equal than others.

Where am I? That thing called money has provided me with a measure of, for me, the utmost significance—and singularity (I shall shortly explain). But it is so very peculiarly inconstant. I would, for this reason, much rather have opted for something like the Smithian labour-commanded invariable measure; only, I can't help saying, I find it to be, even in the idiom of theory, not very operational. It is easy to explain, and I shall do it at once.

In using money as a standard of measure, economics, superficially, does no more than what it, or any other empirical science, does in using any other measure, of number, distance, weight etc., evolved by society. It is, however, in the interest, necessity, the discipline has in hypothesizing about the behaviour of money, in relation to product behaviour, that one can discern the difference between economics and any other science using a measure, or between money and any other measure used by economics; economics is unique in the way it uses money. The distinction relates to something fundamental, to the nature of the money-measure. The fact is that the money-measure is not reliable enough a measure. Hence the need to hypothesize about its behaviour, its value; normally it would be odd, absurd, to contemplate hypothesizing about the value of a measure. This is the necessary condition for economics to hypothesize about money. The sufficient condition is that the measure is involved in the universe of economics in an essential way; in a way, as already indicated, that no other measure is involved in economics, or, for that matter, in any other science. Economics cannot do without a measure such as money. It needs (because the economy needs) a measure in terms of which could exchange ratios be conveniently expressed; whether the measure is paper or platinum is immaterial. But, more fundamentally, it needs (not because the economy needs) the measure as a science, to be able to quantify product behaviour in all its relevant inter-relations. Now, it is embarrassing enough that prices, which is what exchange ratios when expressed in terms of money are called, do not express the changes in the relative values of products in a totally dependable way; that the change in the relative prices are not, so to speak, perfectly "caught". Money, the measure, has an exchange ratio vis-a-vis products as well as products

have exchange ratios among themselves. And one cannot always and perfectly say how much of the changes in the relative prices was caused by the changes in the relative values of products themselves and how much by the changes in the exchange ratio between money and products. This is the micro aspect of the measure problem. But economics does not use the money-measure only for comprehending the relative prices, relative values of products among themselves. It needs the measure also for comprehending, in a common expression, the almost innumerable varieties of products (pins, manure, drycleaning etc.) carrying varying quantitative values (quintal, kilometer, hours etc.) as a single totality (the national product) and, exceedingly more complicated, for comprehending changes, over a period of time, in the value of the totality. This is a derived problem; in fact, the same problem as above, arising out of the changeability of the value of the money-measure itself. But here, the relative prices out of the picture, there is a direct confrontation between money (the measure) and the totality of products, which expresses itself in the price level. This is the macro aspect of the measure problem. The measure problem, it may be noted, is really challenging, both in micro and macro aspects, when changes are involved in exchange ratios between products or in the exchange ratio between money and (the totality of) products; it is inconsequential otherwise. The discipline's interest in the explanation of the relative prices, as prices, is considerable; but one of the reasons (some would say, the sole reason) of that interest is that it provides economics with the only measure that can help it understand the behaviour (for Ricardo, the distributional aspects) of national product. The embarrassment is profound. The Classics acutely, intellectually, irritated by the problem sought for a measure that was constant, or, at least, reasonably constant. Smith's "labour commanded" invariable measure came, intellectually (and empirically), perhaps the nearest to what was sought for; but it was altogether un-operational. The modern economist has found as reasonably dependable a way out as, in the nature of the thing, possible. The way out is the index number. The problem of weightage, and therefore the index number problem, is insoluble. But it would be stupid to bother then. The index number makes do, and that is good enough. The measure is a peculiar sort of a friend. It is a friend of whose inconstancy economics is aware, but a friend it cannot do without.

It is possible, thus, in terms of national product to conceptualize, and theorize on, the behaviour of the economy as a whole. My sister-theories in the other social sciences are so much the poorer for not

possessing such a quantifiable, measurable, standard of reference. Any comparative analysis of, say, alternate social or political systems, cultures, attitudes, ideologies or the like, necessarily involves, for want of a standard of reference, implicit or explicit introduction of value judgment, which, of course, all but makes nonsense of legitimate theorizing. Evidently, there are theories and theories as well as systematization and systematization. So there is also something to cheer. With all its deficiencies the money measure has really been a godsend for me; I am compared with the theories in the other social sciences, so much better off for it.

But why am I still not one among those on the top? I have already indicated the sort of systematization that there has been possible in me, and the basic reason thereof. But there is another reason, or perhaps another two. It will take a while to explain, so I had better not rush.

This thing, economics, which qualifies me and makes me into *economic* theory, is a queer mix-up of subject matter and methodology: it has a fair share of suspect elements of subject matter and of spurious formulations (us, me, dear me!). Paradoxically, the conclusions as regards the suspect elements of the subject matter, considered in the extreme case as defining the subject matter of economics as a whole—human behaviour as a relationship between ends and means which have alternative uses—, have tended to take on a probabilistic value which even the physical sciences, if only they cared for just any kind of certitude, might envy. But, of course, the rules of the game, both of theorizing processes and admissibility of facts, have in such cases been diluted to the vanishing point either by once-and-for-all empirical, or measure, assumptions, or in the most provocative case of all even by assuming away the question of evidence for material truth, proclaiming such unempirical judgement on the nature of economic generalizations as that the basic postulates of economics are obviously true. A distorted conceptualization of subject matter is particularly pernicious since weaknesses in logic are not too difficult to detect while a respectable tradition of clinging on to suspect subject matter does not easily invite a logical point of attack. It is, indeed, not a logical question at all.

Ordinarily, however, discussion on the identification of the subject matter, or on the definition of the discipline, is, in any advanced science, a rather unavailing part of the business, not much worth bothering about. To put it differently, a science is advanced when the question of its definition becomes inconsequential; too commonplace to be consequential, that is. This, indeed, is the most



significant watershed in the evolution of any science. And this happens when the theoretical structure of a science, the inter-related theoretical system, has become sufficiently organic, integrated, to be able to tell spurious from genuine elements of the subject matter, and develop an almost built-in logical mechanism, as it were, to reject the former. Relevant but less integrated groups of elements are, however, not rejected but in due time identified and further developed as sub-systems, branches of the science, more or less independently. The really sticky situation that a discipline can encounter is one where basically different but somehow competing groups vie with each other and make the identification of the subject matter difficult and indecisive.

What am I--about ? This raises a general problem: how to identify the subject matter of a particular science, the group of inter-related facts it deals with ? The group is, of course, an abstraction, but an abstraction necessary to determine which facts are to be invited in. The abstraction, too, implies a definition, the definition of the group. It looks like arguing in a circle altogether. Fortunately, one can talk about a science only after it has become distinguishable as a more or less separate systematized body of knowledge; after it has already demarcated, or begun to demarcate, the group of facts it deals with, its subject matter, from out of the universe of facts. The aphorism "economics is what economists do" is, after all, not all that empty and meaningless.

But, as indicated earlier on, what if a science is still wavering, sometimes dealing with, or emphasizing, one group of facts, sometimes another, not knowing, as it were, the criterion of selection ? What, in other words, if the scientists in that particular science appeared to be "doing several things" ? I should certainly want to be sure if indeed I was not, as a system, made to be doing several altogether different things.

On my part, I will not be making a revelation if I said that, in fact, economists seldom agree. Not even as regards the group of facts forming the subject matter of economics, nor as regards method; if they did seldom agree, it is altogether to be expected. Few economists, to be fair, have approached, the conceptualization of, economics from an impersonal angle. The usual approach has been rather direct, personal, which explains why, as the variety of definitions of economics shows, many conceptualized economics, not as it is but, almost, as according to each one of them, it ought to be. 'Almost', because few of the definitions wholly abstracted from what economics as a discipline has been dealing with. But the procedure did amount to

singling out a particular slant, leaving out several crucial elements, when the correct procedure was to look for a criterion of selection which took care of all the crucial elements organically integrated with, systematized as, the chain of my plural self.

Let me pick up (already referred to) one of the most, perhaps the most, celebrated: study of human behaviour as a relationship between ends and scarce means which have alternative uses. This, rescued from its possible metaphysical implications, may be interpreted as follows. It sounds quite legitimate to engage in the study (explanation) of the, empirical, behaviour of the phenomenon in question. The nature of the means has been defined as being scarce and non-specific; that which enters the process of exchange cannot but be scarce, and money excludes specificity. A choice is involved; the choice is rational, in the sense of self interest being the decisive motive force, altogether expected since exchange cannot but be for gain. Rationality is, in fact, economizing, eminently appropriate both to the context (means-ends relationship) and, linguistically, to the subject (economics). It is only the next step to see that to economize is to minimize the means, what you give in exchange, or, which is the same thing, to maximize the ends, what you get in exchange. In the bargain, the definition already explains the behaviour, the relationship, the process of exchange. The solution, the minimax (here used in the sense of minimizing or maximizing) providing an absolute term of reference, is wholly determinate. And I, theory, wholly redundant.

I am, I find, already treading on exceedingly delicate ground—on my own neo-classical self, the Walrasian system and the rest of it, the apple of the eye of the most sophisticated of my creators. I am conscious (could I possibly be jealous of my neo-classical self?) of the great favours shown me in my this particular garb. But how misplaced; I almost wish I didn't know better: the apparent positivity of linking up means and ends conceals the normative objective of economizing, and the apparent empiricism is at best pseudo-empiricism describing little more than purely mathematical (deductive) relations. Of course, there is nothing inherently illegitimate about hypothesizing rationality, even maximization, although, one has to be reasonably certain that the laws of demand or supply or product behaviour generally needed to be buttressed by a supposedly more fundamental hypothesis, such as, and especially, the maximization hypothesis. But, even if that were the case, the hypothesis must be shown to be true or false. As it is, economics does never assert it to be true (actually it is taken as an assumption but this hardly helps) except as an "on

the whole" postulation with qualifications that all but make the hypothesis meaningless; and, at the same time, unhesitatingly relies on the deductive elaboration of the hypothesis (I refuse to have anything to do with such a prototype). Indeed, the discipline does a lot more: it is so carried away with the implicative power of the hypothesis that (the definition I am reflecting on does precisely this, of course) human behaviour is almost unavoidably conceded the position of being the definitive subject matter of economics. Worse, it injects normativity into an avowedly positive exercise throwing up in the bargain the stuff called optimization which you are free to take as an ideal, as a standard of reference or, if you are not sufficiently awake, as real. All in all, the rationality assumption for man in economics, the way it is postulated and made use of, is about as meaningful as a rationality assumption for bodies would be in physics: at worst, a case of disguised unemployment with high negative marginal productivity; at best, redundant. I feel rather ashamed of belonging where I do.

Perhaps the best thing (for me) is to be what one is (I am), to have the courage to accept oneself (myself); to be a man, as they say. It's a terrible situation. No wonder Planck found (and the stuff in his time was not as neat) economics too difficult. He was almost certainly being polite. But, then, the situation is a little less hopeless than that. There already is an alternative: to realize that economizing, as the possible linguistic filial concept of economics is a red herring. One must turn to the other such concept, the economy. The choice construct, empiricism by proxy, and treating of human behaviour directly as if it were itself the phenomenon to be explained, the mainspring of the choice construct, can have no legitimate place in economics. And my creators have been doing this other thing as well. I should myself be happy though if I am not made to stand on two stools; if, in other words, human behaviour is altogether relegated to the category of explanatory variables in favour of product behaviour as the crucial identity defining the subject matter and, therefore, the discipline of economics.

That 'economy' refers to human economy is absurdly obvious. But does this in itself make the study of the functioning of the economy necessarily the study of human behaviour? Were it so, the study of carrots could also come close to making that claim. Carrots grow in the wild and are also grown by man. They have, as a species, a certain logic of growth, more or less known to the botanist, who, in fact, may be growing them for his scientific as well as culinary and, if he has enough to sell, more directly pecuniary interests. He may indeed be making all sorts of experiments to

improve the yield, the size, the taste, the food value, or, some more esoteric properties, of carrots. But the botanist would frown if one suggested that he was engaged in the study of human behaviour. The question is not whether human beings are more or less intimately and directly associated with the economy, for they obviously are. The question rather is whether products have, like carrots, or the celestial bodies, a way of behaviour fundamentally independent of their association with man. In other words, do they exhibit properties which, when attempted to be controlled or directed by man, refuse to respond in a manner contradicting the logic, or laws, of their behaviour. The type or magnitude of the residue is not at all important. The important thing, in the context, is that only if there were no residue might the study of human behaviour imply the study of the behaviour of product. In fact, there is a considerable residue in the behaviour of product which refuses to be explained by human behaviour. There are, to be sure, areas of product behaviour which, for the purposes of explanation, call for human behaviour. But, obviously, one cannot take the position that economics was the study of human behaviour, even a certain aspect of it, as well as of the behaviour of product.

While the economist is free to define, conceptualize, economics in any way he likes so long as he continues to be strictly empirical, he had better not put human behaviour in the centre of the universe of his science. He certainly can, and must, deal with one or more aspects of human behaviour in so far as they influence the behaviour of product. But he is then, he must remember, treating of human behaviour exactly as he is treating of natural or technological influences on product behaviour. It is the business of the economist to discover what exactly it is in product behaviour which, in spite of, or perhaps because of, the numerous influences, including human influences, working on it, exhibits a logic of its own, and what laws can be discovered from the study of these elements of product behaviour. He needs first (through me, of course) to grasp these laws of behaviour before he can advise how to direct product behaviour. This direction, like the direction by man of natural forces, simply confirms the fact of laws of product behaviour. One can direct forces, not laws. If planets were made to change their course by man, or thrown about from one solar system, or even galaxy, to another, it would not mean that physical laws have been changed. In fact, this will only further confirm, if further confirmation were needed, that the physical laws are independent of human volition or action. Let one deliberate thoughtfully matters such as these, and one is sure to

discover that economics can have little to do with formulations uniquely based on maximization, something ostensibly empirical but really predetermined, normative. This is the only possible, legitimate, conceptualization of economics as an empirical science that it is; a conceptualization which, allowing for the evolution of a theoretical, explanatory, system of analysis as an organic whole, views the discipline as the study of the behaviour of product, product behaviour, of which the study of the behaviour of national product is, of course, but a part. This is also, if one must have one, the definition of economics, a definition (except for the distinction that his conceptualization, in terms of the behaviour of the "wealth of nations", national product, is a species of the genus, the behaviour of product) about as old as Adam Smith.

This would be where I should like to belong. Of course, I would continue to carry the handicaps, that, as theory in a social science, even in economics, I must. But I will have come in my own. And, in any case, I shall be free from any conscience problems—and as far as possible from any complexes. I should also then be more effective as well as more illuminating. I should certainly be generating more genuine policies, and my predictive capabilities should have been enhanced. I can even hope that, with my having been shown as I am with greater clarity than is the case at the moment, no policies will be contrived from out of me; nor non-existent predictive capabilities, wishfully or in desperation, presumed of me. I am economic theory; but I am, if I may say so, theory, first.

## CHAPTER 2

### The Vocabulary of Economics

I propose to examine the implicative power of the following proposition in illumining a question of material truth. The proposition is, *the explanatory potential of a science is a positive function of its vocabulary*. The focus, economics.

The last term of the proposition, (its) "vocabulary", refers to, means, a body of terms, where a term is a word, or a combination of words, with a specific meaning (definition), ideally, without a synonym. Since it has a relation of belonging to a science, implying that words of ordinary usage (which, of course, have synonyms) or terms of another science do not belong to the science in question, the vocabulary of a science has to be viewed as a body of terms the science has developed, terms carrying the definition the science has given them. Such terms are referred to as technical terms. The technical terms of an empirical science, a science concerned with material truth, represent aspects of the behaviour of phenomena, and of the inter-relationships among such behaviour, which the science deals with and which are as the science views them to be. Such viewing, essentially, is an abstraction, an open admission that not everything possibly relevant has necessarily been taken account of; a three-layered abstraction, in fact : what a particular phenomenon is, what is its behaviour, how is the behaviour inter-related with another behaviour. It is, moreover, a changeable abstraction, one abstraction, replacing, or competing with (until, if ever, the conflict is resolved), another; so because it is always possible for a science, always in search, to come by new, controverting as well as confirming, evidence. Since science is explicitly an explanatory (theoretical) system, the technical terms of a science may be viewed as emerging from the attempt of a science to establish relationships between the variables (behaviour) to be explained and the variables relevant to the explanation (the explanatory variables). Any explanation takes the form of a hypothesis which when verified, takes on the badge of theory. But, of course, theories as well as

hypotheses are abstractions, and are necessarily provisional; the difference between a hypothesis and a theory is, essentially, a difference of degree rather than one of kind. Any abstraction needs, for comprehension and analysis, to be expressed as a positive statement, proposition; and technical terms represent the crucial elements (as subject or predicate terms or as relational attributes) of the proposition, although, of course, they basically represent the crucial elements of the behaviour of phenomena the abstraction is about. The proposition I am at the moment examining is itself an abstraction, and the term, one of its crucial elements. The vocabulary of a science, then, is the sum of all such technical terms developed in (by) that science. Such a vocabulary is referred to as scientific, or technical, vocabulary, although the former description is far more appropriate if only because a vocabulary can be technical without being scientific. This, of course, holds for terms as well. As a science changes, grows, so does its vocabulary; and, of course, the terms imbibe the changeability of the abstractions: they may simply undergo a change of definition or may be altogether replaced—in either case, new terms have appeared, although the old ones may, do, carry on a shadow-like existence for purposes of reference or comparison, or, perhaps most decisively, simply because they are there. So the vocabulary of a science has its museum pieces as well as new breeds. Lastly, since science is a theoretical system, in the sense that the various theories are more or less inter-related, the vocabulary of a science also reflects a system, in as much as the various technical terms are more or less inter-related, among themselves. “*Explanatory potential*” is, of course, the capability of explaining, and “*positive function*” defines the nature of the relation, although no value has here been assigned to positivity.

That the proposition does not involve circular reasoning, or does not beg the question, should be obvious from the foregoing; the understandably intimate relationship between a science and its vocabulary does not by any means make them identical, for that would allow the proposition to be read as, *the explanatory potential of a science is a positive function of its explanatory potential* or, less relevantly, *the vocabulary of a science is a positive function of its vocabulary*. Nor, of course, can the proposition read, *the vocabulary of a science is a positive function of its explanatory potential*, since the proposition, *all women are human beings*, could, then, read, *all human beings are women*. But, assuming the fact (existence) of the explanatory potential of the science (or, of the existence of women), the proposition could certainly read, *part of the*

*vocabulary of a science is a positive function of its explanatory potential* (or, *some human beings are women*). What if my proposition had originally been, *the vocabulary of a science is a positive function of its explanatory potential*? To answer the question let me suggest a proposition such as, *the distance covered per hour by a train is a positive function of its speed*, and its companion proposition, *the speed of a train is a positive function of the distance covered per hour by it*. It will be admitted that "explanatory potential" as well as "speed" is a more abstract concept than "vocabulary" or, in the other case, "distance covered per hour." So, after all, my original proposition, *the explanatory potential of a science is a positive function of its vocabulary*, is capable of being handled relatively easily; apart from the fact of my decision to examine the relation in the particular way my original proposition indicates. Of course, all that the proposition itself affirms is that if a science has any explanatory potential it is a positive function of its vocabulary; it affirms an implication between a science having explanatory potential and the science having a vocabulary in a certain specified manner. It affirms, in other words, that it is not the case (the possibility is totally ruled out) that a science does in fact have explanatory potential and yet the explanatory potential is not a positive function of its vocabulary. On the other hand, it has to be clearly understood that the proposition does not affirm anything either about a science in fact having explanatory potential or about the explanatory potential being a positive function of the vocabulary of the science in question, taken separately. For all the proposition cared, both may in fact be (materially) true, both false, or any one of the two true and any one of the two false. Information of the nature of material truth is wanting—absolutely.

But, I suppose, I can supply some information of that nature, and I will supply all the information of that nature that I safely can; and, later, even some that I safely cannot, indicating, in which case, however, a broad and general notion of the unsafe zone. The very first safe information that, I think, I can supply is that there is, in fact, a science, the science of economics; the second, that it (economics) does, in fact, have (which is all that is needed) some explanatory potential; and already, I recall, my proposition has just the requisite information for it (now) to affirm that the explanatory potential of economics is, in fact, a positive function of its vocabulary. Really? For in that case, the proposition, *all short human beings are women*, if given the information that, in fact, there are human beings and that, in fact, some of them are short,



should affirm that all short human beings are women. Which shows perhaps that it has all gone wrong. Actually, nothing at all has gone wrong. For the argument (reasoning), to be valid, must take the form, *if economics has explanatory potential, it is a positive function of its vocabulary—(there is in fact something called economics and) economics has (at least some) explanatory potential—(therefore, the explanatory potential of economics is a positive function of its vocabulary.* The factual information needed is not only that there is in fact something called economics and that it has explanatory potential (which is what I have supplied) but also that the explanatory potential of economics is a positive function of its vocabulary. The parallel reasoning relating to women and human beings must, to be valid, take the form, *if there are short human beings, they are women—(there are in fact human beings and) there are (at least some) short human beings—therefore, all short human beings are women.* But the information that if economics has explanatory potential it is a positive function of its vocabulary (or that if there are short human beings they are women) has not all been supplied (certainly I have not supplied it). Therefore, the question of material truth (whether in fact), the explanatory potential of economics is a positive function of its vocabulary (or that in fact all short human beings are women) remains open; certainly, the argument does not allow this inference.

The scope of examining the implicative power of my proposition does seem (except for the relational attribute as defined in the proposition, which I shall take up later on) to have been exhausted, and the power, for my purposes, rather invisible. But I shall persevere. Already, after all, the examination has provided me with the opportunity to clarify some relevant points, and to ensure that I know what the proposition means—so much part of the implicative power of a proposition. And it gives me an expository framework, which should help me avoid going too much astray. I shall, however, make a change, a change that, I think, I can now make; more formally than I had already begun doing. The proposition now reads, *the explanatory potential of economics is a positive function of its vocabulary.* Also I shall now be more daring; I find that I can't avoid being that, concerned as I am with material truth. The unsafe zone begins.

The first step is crucial, it is always so. My first step, it is really a leap, is explicitly to assume (far better than doing it implicitly, any way) that the proposition is true, materially true. I have evidently not kept my word—have already made, not one but, a lot too many changes. It simply could not be helped. I am perservering.

And this last change ! What else remains ? Why, the unsafe frontier has just been crossed.

But, now, this proposition, *the explanatory potential of economics is a positive function of its vocabulary*, it will be recalled, was, when we were not examining it formally, the conclusion of the argument based on the premises, *if economics has explanatory potential it is a positive function of its vocabulary*, and, *economics has explanatory potential*. Now, if the proposition is (assumptionally) true, it does not necessarily make the two premises also true; they both might be false or any one of them true and the remaining false. Let me (to continue to be daring) assume that the proposition, the second premise of the above-mentioned argument, *economics has explanatory potential* is false. The remaining premise, *if economics has explanatory potential it is a positive function of its vocabulary*, is also (by implication or assumption) false. Of course, the truth of the proposition, *the explanatory potential of economics is a positive function of its vocabulary*, remains unaffected; it is true by assumption. What could all this possibly mean ? One thing that it could mean is that the explanatory potential of economics is little more than notional, and the relational attribute of a positive function is easily satisfied by the vocabulary of economics not doing much credit to what the vocabulary of a science should be (with the implication that economics as a credit booster does little better, but I did not need to make the implication explicit). It could, I find, also mean that the vocabulary of economics is what I have just said about it, and the relational attribute, in the reverse functionality, is equally easily satisfied by the explanatory potential of economics being little more than notional (which, of course, implies that economics should have no compunction of conscience in refusing to be burdened with being called science). As a matter of fact, I have my own views about such matters as pertain economics but politeness forbids me from expressing them as long as it can possibly be avoided. But it can be suggested that the possible meanings I have just been deriving are as well kept in mind. And since I have assumed my proposition to be true I feel encouraged to admit that I am on the same indifference curve whether I opt for the one or the other meaning. But if I felt the urge to be decisive, I shall use another dose of my daring as the price line and use it in a manner (which is the same thing as my having taken into account the counterparts of the substitutional attributes of the two meanings) that gives me the result that the former of the two meanings is the only relevant one. Or, rather, I make the above result on explicit assumption. Of course, I have my

reasons, weak or strong; but stronger than any that I can order to be able to assert for the other meaning. I think (many do, I find) that the explanatory potential of economics leaves much to be desired. Certainly, it does not seem very likely to be the envy of many other sciences. The other social sciences may, admittedly, be said if anything to have something that economics itself is probably not exactly envious about. But, surely, these others cannot even by implication be accused of raising false hopes, which cannot, economics might agree, be said of economics. But, to make my position more understandable, I cannot, on the other hand, pass even a relative judgment about the vocabulary of economics doing or not doing credit to what the vocabulary of a science should be. I have no information even of the type that I have for the explanatory potential of economics. And I know my limitations as an information gatherer; by no means do I feel proud of this, but I could not possibly have not stated the fact.

It is also an advantage of my last (so far ?) assumption that I can, *ceteris paribus*, except myself to be needing, for further examination of the proposition, to be a much less thorough information gatherer than would be the case if I had opted for the other meaning; and time, any way, I began doing some information gathering, too. I cannot promise myself much, though, and, in any case, reflections are not particularly in the habit of being punctilious in such matters of detail; one operates in a rather rarefied part of the skies, and minor lapses are to be taken in good humour. Honestly, my information gathering will be by proxy ; I shall solely go by the information passed me by my past gathering. And, of course, my expository framework remains in tact.

If the explanatory potential of economics is rather feeble, if the vocabulary of economics leaves much to be desired, and if the proposition, *the explanatory potential of economics is a positive function of its vocabulary*, is true, all that appears to remain for me to do is to try to go into the why of the vocabulary of economics being what it is. That the vocabulary of economics emerges from the attempt of economics to explain behaviour appropriate to its universe of discourse has already been indicated. So, if the state of the vocabulary is what it is, there should surely be something not very edifying about the attempt. I cannot, however, rule out the possibility that there is absolutely nothing unedifying about it; instead there may be something about the universe of discourse itself which, despite best possible attempt, does not allow the attempt to fructify as well as the same attempt would in a more responsive universe

have. If so, the explanatory potential is what, in the nature of the thing, it possibly can be, and the same holds for the vocabulary. But, apart from any other considerations (and one such is that I will have got myself caught in a vicious circle), I have already barred myself from taking this position: the state both of the potential and of the vocabulary is assumed to be less than entirely satisfactory. Inevitably, then, the attempt falls short of standards, whatever the standards be. But right here I must be alert—I am close to the boundary-line of another vicious circle, that of the two possible meanings; those that had confronted me quite earlier on: one, that the explanatory potential of economics was little more than notional, the other, that the vocabulary of economics did not do much credit to what the vocabulary of a science should be. I recall the decision already taken, and I am safe: I am barred from having to go into the why of the explanatory potential of economics being what it is. The purpose of invoking the attempt of economics to explain has simply been to keep reminded that the vocabulary reflects the attempt—abstraction and all that: to be able more effectively to examine the why of the state of the vocabulary of economics, not that of the explanatory potential of economics (or not directly). After all, there must be a standard of reference, which, obviously, I cannot possibly generate from within the vocabulary (any vocabulary, for that matter). And I can now proceed; and I will proceed in terms both of the quantitative and of the qualitative aspects of the vocabulary of economics. The qualitative aspects, first.

I cannot help being selective, and shall confine myself to something that reflects on the methodology and empiricism of economics as an empirical science; I should myself think, something fundamental. If an empirical science dilutes the method appropriate to the search for material truth (raising genuinely empirical problems, putting forward hypotheses strictly in conformity with inter-connections among matters of facts as pertain the relevant universe of discourse, verifying the hypotheses directly or, through deductive elaboration of the hypotheses, indirectly), the terms (relating to the concepts and relations; even, indeed, those relating to the matters of facts considered relevant as variables to be explained as well as their explanatory variables) which would be thrown up in the process of theorizing, and to that extent, therefore, the vocabulary of the science in question cannot help more or less correspondingly being suspect and spurious. What can in this respect be said about economics? I shall consider specific examples, of principles as well as (though mainly of) practice. It may be a dead horse (I myself do not

think it is), but it is certainly reflective of the goings-on in economics to be able to have the fun of encountering such a deliciously untelligible formulation as that the basic postulates of economics are obviously true. I shall merely ask a few questions. If the truth referred to is (as it must in an empirical science like economics be) material truth, and if evidence for material truth cannot be (as it cannot for the same reason be) conclusive, is it anything but a contradiction in terms to speak of obviousness of material truth? If the formulation is accepted, is economics anything else but as many deductive (mathematical) exercises as the basic postulates allow? Who decides what are the basic postulates—are they given and fixed (with the implication that economists as a tribe had better quit, leaving the job to be more efficiently done by logicians)?—if they are not given and fixed, how to admit the new entrants (with the implication that the question of material truth, then, becomes too open to be tackled by the formulation)? Meanwhile, with the term “obviousness of material truth”, the vocabulary of economics has undergone a qualitative change. There is also Friedman, inviting no less blunt questions. His contribution to (the qualitative side of) the vocabulary? “Irrelevancy of realism of assumptions.” And economics recognizes the two (Robbins of course was the former) as the most recognizable stalwarts on its methodological issues.

Practice? Dead horses, again? And, again, I myself do not think they are; some of them, indeed, are rather too well and kicking, too. One begins, in economics, with value; I shall follow the tradition. I shall merely ask questions; even dispensing with background material, although the questions, to economists, should be quite backgroundful as well. And I shall be following my own tradition of not answering my questions. Has the neo-classical equilibrium price really (expressing inter-connections among the relevant matters of facts) dissolved the classical distinction between natural price and market price? Is not the Marshallian period analysis a ‘bad’ escape from the Classical natural price? Is the demand-supply construct a theory? Is the maximization postulate an assumption (in which case which discipline’s hypothesis is it?) or a hypothesis in economics (in which case does the neo-classical exercise on price at any stage verify it?) Is, needed, the whole of neo-classical micro analysis a system of analysis (=a chain of analytic constructs) or a system of theories? And, as a round-up (since questions on scope and method are conventionally discussed while introducing micro analysis), is economics a study of human or of product (inclusive, of course, of inputs and outputs and the aggregate, national product) behaviour? If the

answers suggest confusion or contrivance, the vocabulary developed in the process cannot be entirely what it should be—reflections of explanatory inter-connections among the matters of fact relevant to the universe of discourse of economics, a body of scientific terms.

The qualitative picture as pertains the so-called macro portion of the vocabulary appears to me to be, if I may say so, qualitatively different from that of the so-called micro portion. And the basic explanation is that, in general, the terms thrown up in the process there are, essentially, truly scientific. Not that there are no purely technical (analytic=non theoretical) terms in this portion of the vocabulary; no science can do without such terms while hypothesizing and verifying. But such terms, with exceptions to be indicated later on, are handmaidens to rather than, as in the neo-classical portion, substitutes for the scientific terms. It is interesting to note that the vocabulary of Classical economics, pre-eminently macro, is almost entirely free from handmaidenly substitution.

Now what is it precisely that I have in mind in making the distinction between purely technical and genuinely theoretical? I cannot here begin to engage in an elementary (which is what I can) exposition of scientific method. But I can broadly indicate how I look at the matter. I am aware that purely analytic (non-existent in the empirical world) concepts are not conspicuous by absence in the physical sciences; and they are there of course not without purpose. But no physicist will be confused about what they are; he will certainly not confuse them with real (empirical) concepts. The need for such concepts (and, indeed, for wholesale models—conceptual constructs not being very fussy about empiricism—based on such concepts) may, if anything, be greater in economics: too many unknowns, difficulties arising out of diversity of behaviour, difficulties of quantification and measurement, and the like. But this could only mean that the hypotheses, theories, in economics are to be accepted as being not very reliable. It is, for the same reason, understandable that the discipline takes recourse to purely analytic constructs based on, in some sense, 'ideal' assumptions, to be able to appreciate the possible range of behaviour, as a standard of reference for its genuinely empirical hypotheses.

But such constructs can by no means be substitutes for, or even allowed to be confused with, theories proper. Nor is it scientifically legitimate to avoid any inherent difficulties of the discipline by contrived elegance such, for instance, as that made possible by treating of human behaviour as the definitive subject matter of economics, and of the maximization postulate as an empirical reality.

A science (in the sense that it can't be stopped) can do all this, and worse; but the empirical inter-connections are what they are not what such doings of the science make them appear to be. Just as one can validly infer only what is implied, one can truly hypothesize only what the empirical inter-connection, in fact, are.

And now to quantity. I already have, I confess, a 'hypothesis' about it; in a way, actually implicit in the qualitative appraisal of the vocabulary. The hypothesis is that the purely scientific terms in the vocabulary of economics are too few. As is my wont (already sufficiently demonstrated), I shall proceed from principles to facts. If the vocabulary of economics is qualitatively deficient, for the reason that it has a surfeit of purely analytic terms, it might have some quantitative implications for itself. It would not matter if the purely scientific terms covered all that needed to be covered; the quantitative load of the purely analytic terms could then, in part, be ignored as a superfluity. There are, however, two reasons why this is not in fact likely to be the case. The first, I am afraid, is the superfluity itself; it does, definitionally, inject a certain disproportionality in the vocabulary as a whole. Even the disproportionality would not matter, if much of the analytic portion are living a shadow-like existence. As it is, the terms there are exceptionally active. I should myself think that many of them deserved to be declared museum pieces. In the event, the vocabulary cannot help carrying a technical rather than a scientific look; rather unusual for an empirical science. Could it be that there is something peculiar in the nature of the subject matter of the discipline which forces such disproportionality? Or, perhaps, as some of my straight remarks as well as insinuations would indicate, the choice of the subject matter has been rather peculiar. If so, it is a more worrying matter in itself rather than an attenuating circumstance. In any case, the disproportionality would appear to represent a case of having to have less of one thing to be able to have more of another. But I have already passed on to the second reason. The involvement with analytics rather than with theorizing proper is so pronounced in the neo-classical micro economics that it has, over time, tended to spill over to the macro portion as well. The phenomenon is quite interesting in itself. The discussion on the price problem during the Classical period was quite sharp and even high-brow. But the real controversies were directed at empirical problems, and I do not get the impression that price theory then commanded a prestige that the theory of the behaviour of national product could envy. The classical price theory, to me, is more empirical than the neo-classical

one. It is, therefore, a hypothetical question, but if the Classical price theory were as nearly devoid of genuine empiricism as I consider the neo-classical price theory to be, I would still not expect the analytics of the classical price theory to have fundamentally affected the theoretical character of its macro theorizing. The modern, neo-classical, price theory, on the other hand, has come (underservedly, but that is a different matter) to be considered by the fraternity to be the acme of theory (something, again, interesting in itself) and, understandably, the rest of the discipline could not help (fortunately, not wholly) adopting the micro analytic procedure of model construction of a not altogether genuinely theoretical character. For quite a considerable time, indeed, the neo-classical micro economics was economics. This shift in methodological emphasis has accounted for a body of purely technical (analytic) terms in the macro portion as well; and it is a plausible surmise that but for this shift the intellectual preoccupation of economists would instead have been directed at more genuinely theoretical endeavour. The vocabulary of economics, therefore, is poorer by not having had the terms it otherwise would have.

But I seem to have forgotten my proposition, either in its old or re-incarnated form; I had still to examine its relational attribute. The relation, as already noted, is positive, although no value has been assigned to positivity. So, the positive relation between the explanatory potential and its vocabulary may well be exceedingly significant, or it may well be worse than insignificant. Now, before I allow my own predisposition to go, material truth-wise, a-spree, I had better reflect on what could possibly make for one or the other. All the circumstances, save one, that I can think of convinces me of there always being an exceedingly significant positive relation. The one, the only one that I can think of, that includes itself out concerns the museum pieces; those that, as we saw, live on without really living, the shadow-like existences. It is curious, isn't it, but the more I think about it the more forcefully it registers, like what I have surmised. I shall explain. Now, the vocabulary of economics (of any science) is made up, so far as I am concerned, of what I have asked it to be made up of—the museum pieces as well as those really alive and kicking. So, if it so happened that the museum pieces were the only (or about the only) ones which could (would) make the explanatory potential worthy of a science, the vocabulary (as per definition) will be anything but wanting, and yet the explanatory potential will be nothing but wanting. It will be noted that, apparently paradoxically, it can never be the case that the vocabulary



is nothing but wanting, and yet the potential the envy of others. For if wanting, the vocabulary will, definitionally, be wanting as a whole (taking account of the museum pieces as well as the in-fashion ones); and if the whole is wanting, I can think of nothing that can make the potential great.

The broad contours of my threatened spree already being exposed, I might as well have done with the modalities. Not exactly an upheaval, but quite some painful operations would appear to be in order for the vocabulary of economics. The vocabulary might or might not like to (or bear the pain of) having to make its present-day museum pieces change places with the other ones; but if its explanatory potential is in fact rather lowly and if the immediately foregoing implicatory exercise valid, well, the vocabulary cannot escape having to allow its museum pieces to sit at level with the rest. Not all of the museum pieces, I suppose; and, although I asked for it, I am here rather in a soup : how to tell which? Fortunately, the modalities could also be broad, and I now feel less uncomfortable. My basic prescription (I mean the prescription derived from my implicative exercises with, I have not concealed, tinges of my darings on questions of material truth) is that the vocabulary might, with an open mind, begin to have a re-look at itself. This, I expect, should indicate how, and how far, to go about the business : which of the museum pieces deserved to be activated. But if I were myself asked to be less evasive on the issue, I might, hopefully, try partially to concretize the picture, more in terms of categories than of individual pieces. I will be brief. In fact, I will simply name some of the likely categories and pieces. First, the classical pieces : the conceptualization of economics as a science, incorporating the Ricardian emphasis; the non-human subject-matter of economics; empirical theorizing; the distinction between natural price and market price; proper appreciation of the nature of the demand-supply construct. Secondly, the Marxian; although I am not sure if even condescendingly have the terms of economics Marxian even now been formally admitted in—in the vocabulary of economics. Is “socially necessary labour” all too metaphysical, or “laws of motion of capitalism” too esoteric, or class relationships too irrelevant ? Thirdly, the Wicksellian, which I mention with a single motivation, viz., that following Wicksell the Walrasian general equilibrium system may be looked into deeply enough with an empirical stance (rather than, derived though it is from the neo-classical theory, the purely analytical stance it exhibits) to settle for

good what the system signifies for the theoretical system of economics.

That was what could be gotten from implicative power; and from the past acquisitions. But a science grows and so should economics. There is no reason why the vocabulary of economics cannot be forward looking. This, however, would appear to demand a more direct approach. And nothing, I think, can be more direct than approaching the vocabulary of economics directly. Can the vocabulary of economics itself suggest something to make it less unedifying as a scientific vocabulary than it is? I think it can. But not in the manner that would appear to come to mind at the first blush, viz., by asking for a qualitative change. For this would amount to asking for enhancing the explanatory potential of the discipline; too direct, too obvious, too circular. A more reasonable (practicable?) alternative would be to ask for correcting the quantitative disproportionality. Of course, this, too, can be said to amount to be asking for a qualitative change; Hegel's nodal point is clearly involved. But the suggestion has the advantage of being discreet (apart also from being not too direct, not too obvious, not too circular), and more effective. More effective, because the precise manner of making a beginning by way of concrete steps can be hoped to be indicated; something just not possible in asking directly for a qualitative change, if only because it is so much more difficult to split up quality—the explanatory potential. Naturally, I cannot begin to do more than indicating how the beginning could be begun.

I cannot help having the benefit of hindsight. Imagine a world which, while knowing too well all about people purchasing more of a commodity when its price declined, is totally unaware of what is in our own world too well known as income and substitution effects. And suppose that someone had been put on the job that I have just now put myself on. What could he possibly do? I can tell what (to mention just one thing) he could not possibly do. He could not start coining words for the vocabulary from out of thin air. He could (to tell what he could possibly do) go a step backwards, or, indeed, two steps. He could think up problems. If he were perceptive (necessary condition) and lucky (sufficient condition), he might stumble on the phenomenon (he could not possibly stumble on it if it were not there) that prices behaved (qualitatively, as it were) in one way on one occasion and rather differently on another. He might wonder, and ask why. Already, he might feel propelled on to begin to search for possible answers. The next step has been taken. It would only remain for him to be certain as to which of the possible answers he

had thought up was the most plausible. Meanwhile, a few terms would have emerged, the vocabulary widened and, if the answer came to be solving real-life problems of the relevant type, deepened: the explanatory potential will to that extent have acquired strength. An anti-climax? The fact is that there are no short cuts. And the implication, that people may not be searching enough; and all that is being suggested is to be so in respect problems as well as solutions. The fortunate (?) thing is that in the universe of discourse relevant here, there is not much room for complaining about absence of problems. For purposes of illustration (and this, incidentally, takes me to the end of my journey, through the unsafe as well safe zone), I shall mention some that appear to me to be deserving of attention.

The first to be mentioned touches on problems and solutions rather indirectly. The methodological sector of the vocabulary of economics might admit in a lot more of the terms relating to logic and scientific method than what seems so far to have been considered wholesome. I am personally of the view that acceptance of my suggestion will remove some very acute and fundamental deficiencies. How easy? Yes, it is.

The second touches on problems and solutions rather implicitly. The subject-matter sector of the vocabulary might begin developing terms such as, and beginning with, product behaviour (the study of which might be considered for acceptance as the definitive characteristic of the discipline's subject matter as well as its definition), unit product, industrial product, sectoral product and, of course, national product (international—not inter-national—product?). Quite a few of the present-day terms will still do, but since human behaviour will then have been relegated from being the focus to one of the explanatory variables, I should be surprised if a whole set of new scientific, and analytic, terms did not emerge in the process of tackling the behaviour of this, I think, many-splendoured stuff. How easy, again? I'm afraid not.

The third entails a slight re-thinking on the treatment of demand in non-perfect competition pricing as developed since Joan Robinson and Chamberlin threatened (and but for Keynes might have succeeded) to make identities of the theory of the firm and economics (a greater contraction than the three revolutionaries of marginalism had, since they had made identities of pricing as a whole and economics). I do think that demand is really not as emaciated a partner (with supply) as it has there been made out to be. And I

should also think that the vocabulary of economics might find the prospect quite exciting.

The fourth is, unless the discipline decides to quit business, inescapable. I have, of course, inflation in mind. The vocabulary is in a rather pitiable state here. Cost push-demand push (the eternal demand-supply construct, of course, and in itself not at all to be despised) confabulations cannot deliver the goods. It is an uphill task, though, and, in a manner of speaking, the discipline can do with, like that of depression, a Keynes of inflation. Wonderful prospects for the vocabulary, too.

That would, I think, also (I am already on the fifth) make the understanding of the process and logic of economic growth considerably more illuminating than it at the moment is. Two Keyneses together could certainly do that. I should have wanted to say a word each on the special problems of development in the so-called underdeveloped countries and of regional imbalances in growth in rich and poor countries more or less alike—and also on the legitimacy of the discipline's present-day rather tangential handling of wider social institutions and attitudes; but I would rather not, since that would be a lot more superficial than what I have already been.

## CHAPTER 3

# Economics as a Bastard Science

There is only one way to be science—to be a systematized body of knowledge; and only one way to be knowledge—to be true. Truth, however, may be material (about the empirical world) as well as formal (validity of argument). Science, therefore, divides itself into formal logic including pure mathematics, and the empirical sciences; the latter broadly divisible into the physical, the biological, and the (human) social sciences. Economics, of course, is a social science. Knowledge, fundamentally, is one and indivisible. And yet the very dictates of the growth of knowledge, at once the strength and weakness of man, the almost unlimited bounds of human curiosity and the rather limited powers of apprehension and assimilation of the individual human mind, require that knowledge is made divisible, branched off and, in effect, separated. The divisions can never be complete nor the separation real, and the truly great scientist is seldom the narrow specialist, but one who can apprehend, as far as he can, the totality of human knowledge. Now, except at a very elementary level, knowing entails explanation. Indeed, every belief, in a sense, is an explanation. But it is when we pass from the universe of beliefs to the universe of relations—relations among beliefs themselves—that explanation becomes the focus. The crucial element in the transition from knowledge to science is the possibility of effecting a system, an organic correspondence, among the set of relations relevant to a particular science. It is not difficult to see that the greater the specialization the more systematized the body of knowledge. Since every individual science concerns itself with a specific group of facts, the facts in a group are, definitionally, more inter-related with one another than with facts outside the group. So must the beliefs, and the relations among beliefs, as pertain the group in question be more inter-related with one another than with those outside. Systematization of knowledge cannot but enhance the explanatory potential. As an ally, a body of suspect beliefs cannot be trusted, and a loosely inter-related body of relations, theories,

may prove to be worse than capricious. I have said that economics of course is a social science. What I meant was that if economics was a science it must be a social science. Indeed, the sole purpose of these reflections is to try to ascertain if economics was in fact a science. I already have a predilection; and even at the cost at the very outset of giving out the fun of the story, I would say that I consider economics to be rather peculiar sort of a science, an unusual cross between formalism (deduction) and empiricism, with a tinge of value judgment soaked in. In a word, I consider economics to be a bastard science. Of course I shall explain. I shall not, however, engage in anything like an exhaustive treatment; my purpose is simply to prompt a possible re-look at the nature of our discipline, and what I propose to say should indicate my justification for the re-look.

While deduction is a necessary accompaniment, a vital instrument, of search for material truth, economics would appear to be using deduction almost for its own sake. The reason why no empirical science can do without deduction is about as obvious as the fact that no science can do without language. Propositions are the vehicles which carry beliefs, and all empirical (inductive) generalizations (hypotheses, theories) are but beliefs. Deduction is concerned with validity of reasoning, and reasoning proceeds from logical relations between propositions. It is true that deduction is not concerned with material truth, but search for material truth at every step requires that deduction is made use of in order to ensure validity of reasoning. How an empirical science states a problem, how it puts forward possible solutions to (hypotheses on) its problems, and how it attempts to verify the material truth of its hypotheses all involve reasoning. Also what a particular proposition, or a set of propositions, may not indicate at the first blush, may be clearly indicated by its deductive consequences. The ultimate desideratum of (material) truth value is, of course, what the relevant empirical inter-connections in fact exhibit, but the deductive consequences allow for a wider range of possibility for looking into the empirical inter-connections. The significance of deduction for empirical science cannot be over-emphasized. No empirical science, from the standpoint of explanatory potential, can be rich enough without being able to use pure mathematics (and formal logic generally) well enough. Economics has been having its share, and it will do well to try to enhance the share as far as possible.

But what is possible has a qualitative as well as a quantitative limit. I need not rub the point that it is possible to make indiscri-

minate use of pure mathematics, although I cannot assert that many practitioners of the discipline do not use bulldozers where spades might have done. My main contention concerns the qualitative limits of formal logic and pure mathematics, since it is my view that economics has tended to develop a snug satisfaction from the elegant logical results its use of mathematics has made possible, to the extent in effect of relegating the question of material truth to a secondary position. It is ironical that the Classical economists, especially Ricardo, have by many been supposed to be deductive economists when, in fact, they were sound empiricists. They made use of logic as appropriately as any empirical science should; indeed, they might, with advantage, have made use of mathematics, something they did not, and perhaps could not, do. Pure mathematics had off and on been made use of for economic analysis both before and after. But the point of methodological departure was associated with neo-classicism, with the Walrasian general equilibrium system having come to be the decisive influence. The crucial element in determining the new role of pure mathematics has been the rather eager quest for determinateness—determinateness of equilibrium. Now, equilibrium itself has been an old point of reference as well as a technique of analysis for economics. The very conceptualization of the competitive mechanism admits of equilibrium, an equilibrium which more or less corresponded with social good. The necessary condition was self-interest (perfect competition), which at once allowed for the comprehension of equilibrium and for the equilibrium to coincide with social good. As soon, however, as free expression is assumed to be to a greater or lesser degree wanting, the conceptualization of equilibrium undergoes a change; certainly, a divergence appears between the purely economic equilibrium and social good.

Let us now allow for a change in the necessary condition. It would appear that it is rather difficult to bind self-interest in terms of any two limiting cases, corresponding to pure monopoly and perfect competition. It is, however, easy to bind it in terms of a single definition: maximization. If we now allow for maximization, and allow it to be perfectly freely expressed, the consequent conceptualization of equilibrium may still be the same as the original one, or it may not, depending on whether the original conceptualization had, implicitly, defined self-interest as maximization. The same holds for the correspondence between the purely economic equilibrium and social good, except perhaps that social good now acquires a more definitive character: nothing else could be better for the society. If

we now go back a little, and allow for imperfections in competition, the divergence between the purely economic equilibrium and social good will be sharper and, certainly, more sharply defined: even if the society was not already aware of its ideal standard of reference, economics will have given it the possibility of adopting such an ideal. But, of course, the society may still opt for an altogether different ideal, or it may even not have one at all. However, it is not necessary for me here to go into the question, including the question how a society could at all come to have any particular ideal.

We may now further examine the nature of the difference between the original and the new conceptualization of equilibrium. There obviously is a qualitative difference as well. The new conceptualization, the one with maximization, is, empirically, less acceptable than the one with self-interest without necessarily uniquely defined in terms of maximization. Maximization cannot but be accepted as a genuine empirical behaviour inasmuch as it is a perfectly (scientifically) legitimate postulate or hypothesis on a possible human behaviour. The question is whether it is in fact true: it is, on available evidence, so much easier to accept a broad and general self-interest as true than to accept maximization as true. I must first, however, take account of the difficulty with a proposition carrying self-interest as its crucial term. Clearly, one cannot proceed unless self-interest has been defined—as unambiguously as possible. Supposing we force the term to have two limiting cases, after all: the range being given by maximization, on the one hand, and something (marginally?) short of self-denial or, at any rate, absence of (i.e., non-negative) self interest. We should then say that what the purely economic equilibrium in a particular situation is obviously depends upon how robust or weak (how close to maximization) self-interest is and how free competition is. The social ideal standard of reference will stay apart, except, of course, in the situation where self-interest is maximization and competition perfect. The point is that economics as a science cannot tell what its equilibrium in fact is unless it can tell what in a particular situation the nature of self-interest is and what the nature of competition is. Maximization allows for a neat definition, but the material truth of maximization cannot, at any rate on the demand side, have a very high degree of probability. A broad and general self-interest might not allow for an equally neat definition of equilibrium, but it would appear to have a very high degree of probability. I myself think, however, that the choice before economics is not too difficult in that the purely



scientific considerations should tilt the balance in favour of accepting the empirically far truer broad and general self-interest than maximization. Also, conceptually, the former does indeed allow for as neat a definition of equilibrium as maximization. The only difference is that economics would then have to be prepared to admit a far more diverse spectrum of possible equilibria: the relevant equilibrium will be what the nature of self interest makes it to be. And this, I think, is what as a science economics can, under the circumstances, be expected to tell. After all, the same difficulty arises as soon as we allow for competition to be less than perfectly free: one cannot precisely tell what the relevant equilibrium is unless one can tell what precisely the nature of competition is. The difficulty in both the cases is to be faced up rather than assumed away. The discipline has already done so as regards degree of competition; it is time it did so as regards degree of self-interest as well. Economics might still retain the equilibrium given by maximization as an analytic standard of reference, to be able, first, to comprehend, and compare it with, non-maximization equilibrium situations, and secondly, to use as many of its deductive consequences as help the discipline in looking for and determining the relevant empirical inter-connections. But limiting myself to the main theme of these reflections, the position is that economics, in confining itself to the logical elaboration of the implications of the maximization-based equilibrium has, in effect, allowed itself to be loosened off from the category of an empirical science and, to that extent, come close to being a queer cross of formalism and empiricism. Naturally, this inference would hold only for equilibrium analysis in economics. Only, economics is equilibrium analysis !

But I must go a little further into the question of the empirical loosening off of the discipline. It is difficult to deny that quite a bit of the empiricism retained in economics is not altogether free from value judgment. The discipline has no doubt tried to separate the social ideal from the purely economic equilibrium. The fact, however, is that the way economics has allowed itself to grow makes it extremely difficult always to separate the two. Notwithstanding its having a welfare department as distinguished from its positive department, the element of value judgment is more pervasive than what one might like to concede.

All in all, economics, to the extent it relies on maximization, represents a case of pseudo-empiricism, and, to the extent it is tinged with value judgment, a case of pseudo-positivism.

## CHAPTER 4

### Causation in Economics

I propose in what follows to examine if, in what sense, and to what extent economics as a science establishes, in its theoretical (explanatory) system, relationships which can be called causal. I begin by noting what I do not consider to be relevant to my term of reference. First I do not consider it necessary, for my present purposes, to go into the question whether there is one, and only one, principle of determination. The question belongs to the theory of knowledge. But it can be said that while adherence to the doctrine has, traditionally, not been scanty, modern science, especially physics, looks askance at it. This change in appreciation of its relevance would, I think, allow a presumption, *viz.*, that the degree of adherence to the doctrine is a negative function of scientific advance, so that a relatively strong adherence to the doctrine in a particular science should be indicative of the science in question having a relatively weak explanatory potential or predictive power. Secondly, I do not here consider it necessary to go into the question whether a science must include causality as one of the forms of establishing relationships in its explanatory system. Already, physics, certainly modern physics, does not go about its explanatory business in anything like a clear adoption of this form. I am, on the otherhand, not at all suggesting the other extreme, that the form has been, or has to be, discarded in principle. Science is after explanation, and explanation denotes meaningful relationships, relationships between behaviour to be explained and the behaviour in terms of which the former is in fact explained. The behaviour to be explained implies a problem, of which the solution is in the form of one or more hypotheses, suggested solutions. The hypotheses are then sought to be verified to be able to say which of the relationships, as embodied in the respective hypotheses, was the most relevant one, the theory of the behaviour in question. To be able for a science to carve itself out as an independent theoretical (explanatory) system, however, it is necessary for the various relevant relationships, theories, in it to imbibe, and exhibit,

an inter-linkage among themselves. Actually, it is bound to be so inasmuch as if, as must be the case, the group of behaviour which a particular science attempts to explain has a more or less common attribute, the explanatory relationships must necessarily also have a common attribute, affinity. Clearly, neither the group of behaviour nor the corresponding group of theories in, say, physics will have much in common with the group of behaviour and the corresponding group of theories in, say, economics. It is this inter-linkage among theories of a science that enables it to conceptualize its theoretical system to be made up of certain fundamental laws (of the nature of axioms) and those that are derivable from the former (of the nature of theorems), in a continual process of realignment as well as reassessment of its theories. The nature of the explanatory relationships can, in a certain sense, be said to be fundamentally the same while in another sense it is possible to speak of differences in the nature of the relationships. It is, clearly, in the latter sense that causation can be distinguished from any other form in which a particular relationship could be established. It is already clear that any differences properly relate to the form rather than to the nature of explanatory relationships : that which so connects the behaviour to be explained with some other behaviour that the latter illumines, explains, the former. Naturally, the connectedness in empirical sciences, is not formal or contrived but rather what, in fact, obtains in the world of reality. A hypothesis no more than expresses what there already is there, and a hypothesis is (materially) true or false precisely according as it succeeds or fails in expressing what there already is in the relevant universe of space-time. It should also be obvious that the connectedness cannot be fickle ; if it were so hypothesizing, and, therefore, science, would be ruled out as a human pre-occupation. In other words, the fundamental attribute of connectedness, and, therefore, of the nature of explanatory relationships, is the attribute of invariance ; explanatory relations denote, are, invariant relations. Finally, it should be obvious now, first, that causal relations also describe invariant relations, and secondly, that causal relations need not necessarily be the only invariant relations. It is the business of science to reflect on what the particular invariant relations in a particular case (and for the science as a whole, in all possible cases) are, and to acquire, and adduce, evidence to verify its relations in terms of material truth ; whether the invariant relations are of a causal or any other form is immaterial so far as the need to verify is concerned.

That is why scientific laws (theories) are of the conditional form denoting a certain relation between the behaviour to be explained,

say,  $q$ , and the behaviour with which the former is explained, say,  $p$ . The relationship itself could be one of the following three types : " $q$  only if  $p$ " (or " $\text{not-}p$  implies  $\text{not-}q$ ", or, which is the same thing, " $q$  implies  $p$ "), which expresses a necessary condition in the sense that  $p$  is a necessary condition for  $q$  ; or " $q$  if  $p$ " (or " $p$  implies  $q$ "), which expresses a sufficient condition in the sense that  $p$  is a sufficient condition for  $q$  ; and " $q$  if and only if  $p$ " (or " $q$  implies  $p$  and  $p$  implies  $q$ "), which expresses a necessary and sufficient condition in the sense that  $p$  is a necessary and sufficient condition for  $q$ . Two points must be noted here. A necessary and sufficient condition is not simply an addition of a necessary condition to a sufficient condition ; the condition ( $p$ ) must satisfy both requirements (for  $q$ ) in itself. This really means that if  $p$  is a necessary and sufficient condition for  $q$ ,  $p$  and  $q$  are equivalent. The second point (which follows from the first) is that if  $p$  is a necessary and sufficient condition for  $q$ , the argument (since  $p$  and  $q$  are equivalent) can be reversed ; one can get back from  $q$  to  $p$ . In other words, if one knows  $q$  one knows  $p$  as well (Exactly as the propositions, *No Cats are dogs*, and *No dogs are cats*, by virtue of implying each other (being contained in each other) are equivalent. But it may not always be possible, or even necessary, to establish such a unique relationship. If one wants to know which particular germ was responsible for a certain disease (in the absence of which the disease will not be there), it would be enough to look for a relationship which satisfies only the necessary condition. Similarly, if one wants to know what particular stimulant would restore a certain mental or physical state (by applying which the mental or physical state will be restored), it would be enough to look for a relationship which satisfies only the sufficient condition. It is also possible to conceive of a sequential relationship such that  $a$  (necessary or sufficient or both) is a condition for  $b$ ,  $b$  for  $c$ ,  $c$  for  $d$ , and so on, so that  $c$  is the immediate, or proximate, condition for  $d$ , while  $b$  and  $a$  are remote, and still more remote, condition for  $d$ . It may also be noted that while it is possible to conceive of plurality of behaviour, such as  $k$ ,  $l$ ,  $m$ ,  $n$ , being (one or another) condition for, say,  $w$ , it is more likely than not that a more thorough inquiry will reveal that there was, after all, only one behaviour which was the relevant condition for  $a$ . It is, of course, possible, given what one is after, to remain satisfied with the plurality rather than go in for a more thorough inquiry into the nature of the relevant relationship.

We have seen that the only relationships which are meaningful in science are the relevant invariable relationships. Now, the rela-

tionships which have been described above (and these are invariant relations) may possibly be considered as causal relations, or they may not be so considered. If they are, the distinction between causal relations and relations which are not causal vanishes almost definitionally; one may then speak of  $p$  being the necessary, or the sufficient, or both necessary and sufficient, cause of  $p$ . But it would be closer to the layman's understanding as well as to the traditional view of the matter to retain the distinction, so that only the third of the relations, viz., where  $p$  is both the necessary and sufficient condition for  $q$ , might come closest to what a causal relation could signify. We already know that such a relation would make  $p$  and  $q$  equivalent, implying (which also we already know) that one can be inferred from the other. The difficulty which this way of putting the matter is that science is after unique determination without having to get involved into the question of cause and effect as ordinarily understood. Unique determination is, however, possible even when the necessary, or the sufficient, condition as well as when both the necessary and sufficient condition is satisfied. Indeed, even though science might be supposed to be after conditions which are both necessary and sufficient, it may be quite satisfied with the sufficient conditions for what it wishes to establish. The ordinary (traditional and laymanish) meaning of causation is expressed by a unidirectional, one-sided, dependence between  $p$  and  $q$  such that  $q$  is the effect of  $p$ , the cause; even that there is a genetic connection between  $p$  and  $q$  such that  $p$  produces  $q$ . Speaking in the idiom of functional relations (which are indeed the vehicles of expression for explanatory relations), one could say that while functional relations, in general, are reversible, causal relations, as a minimum, are irreversible, asymmetrical. It is possible to use functional relations to express relations which are causal as well as those which are not, but I am not sure if functional relations can express genetic connection. If so, it is important to realise that causal relations represent a highly restrictive form of invariant relations. Consequently, it would be more difficult to establish, as a principle, that there are causal laws than to establish that there are invariant relations; causal relations imply invariant relations, but not vice-versa. Causal relations, moreover, are not only a sufficient condition for explaining behaviour, any rigid adherence to this form of explanatory relationship is sure severely to inhibit the explanatory potential of a science. This, indeed, must be the basic reason why modern science cannot tie itself down to this form of search for explanation. Just as a less than thorough inquiry gives the illusion of plurality of causes, a science might, for

the same reason, carry the illusion of having established a causal relation, when the relation, even if dependable at all, may only be invariant. A science having a bias for establishing causal relations cannot avoid being in the worst of both the worlds; it cannot take advantage of invariant relations which are not causal, and it cannot, in the nature of the thing, discern wholly dependable causal relations. The apparent conflict between the layman's usual manner of explaining (comprehending) events in terms of cause-effect relationships and a science's rather chary disposition towards it is also, basically, explainable with reference to the requirement of a science's discipline to have all its theories to converge to (and into) an integrated system as against the layman's unconcern for such a requirement; apart also from the fact that the layman absorbs the more or less obvious, while science has to go deep enough to be able to come out with an infinitely more complex as well as more varied, pattern of connectedness.

To say that science is after discovering invariant relations implies the belief that there are, in fact, invariant relations; by the same token, the attempt to establish, the more restrictive, causal relations implies that there are, in fact, such relations. But whether it is one or the other, science proceeds by making inductive generalizations, which is the method of arriving at general or universal propositions from the particular facts of experience. It is possible to be excited (as J. S. Mill and his predecessors in the line, Hume and Bacon, were) about certain methods (the so-called Mill's methods) which are supposed to enable science to discover, and to demonstrate, relations which are causal. The fact, however, is that none of such methods can, with whatever benefit (and the benefit is by no means unbounded), begin to be applied without there already having been a hypothesis on the problem in hand. Also, whether a particular generalisation, or hypothesis, pertains to causal or, the less restrictive, invariant, relations, it yields, through deductive elaboration, certain inductive inferences which enable science, through gathering further facts of experience, to verify the generalization. It is not difficult to see that, everything remaining equal, it is more reasonable to expect exceptions to causal than to the more general invariant relations. Lastly, whether the relations are causal or invariant, modern science, most notably quantum theory, has tended to rely on statistical regularity rather than on laws (causal or non-causal) determining individual events. It need hardly be added that no matter what the nature of relations, causal or invariant, or merely statistical regularity, the preoccupation of science (empirical science) is genuinely empirical,

entirely free from any definitional or contrived connectedness between the behaviour to be explained and the behaviour in terms of which the former is explained.

I may now further spell out, with concrete examples, the difference between causal relations and invariant relations which should indicate sub-classes of non-causal invariant relations, as well. What does the thing, 'water', signify? It will be agreed that it is something imbibing a constant, invariable, conjunction of certain specified properties. Can we speak of water being caused by something? I should not think so; at any rate, it will be rather forced to apply the description causal—and even then it will be a case of multiple causation (plurality of causes), and there is no knowing how to arrive at a single cause. It would perhaps be more reasonable to consider water as a classificatory comprehension of reality. Take another example. Does "iron sinks in water" indicate a causal relation? No, because several other conditions must be there to allow iron to sink in water. Indeed, a search for these other conditions will enable one to apprehend more fundamental relations (and, naturally, more fundamental laws of behaviour). Here is one example how a science (and it cannot be an advanced science—with strong explanatory, and predictive, potential) can pat itself at its back for having established a causal relation when, in fact, it is no more than a surface appreciation of reality. Nor indeed can a law such as Ohm's law be understood as expressing a causal relation, despite the possibility, indeed, the fact, (and the consequent illusion that there is a causal relation) of changes in electric current going together with changes in the potential difference. Nor, of course, do such comprehensive theories as the theory of gravitation express a causal relation where, indeed, not all possible relevant instances are capable of being directly observed. I have failed to give even a single example which I could unambiguously call causal. On the other hand, no one can be stopped from considering any general case of invariant relations as a case of causal relations. I can think of only one category which may be recognised causal (as well, of course, as invariant), where a behaviour,  $p$ , is both the necessary and sufficient condition for another behaviour,  $q$ . Only, I am then constrained to say that, since  $p$  and  $q$  are equivalent, and one can be informed from the other, I do not see a unidirectional, one-sided correspondence, much less a genetic connection. I would myself suggest (as indeed I have already implied) that any science which has a significant number of causal relations in its theoretical system has better re-examine its causal relations; I promise that it is very likely to be strengthening its explanatory potential as

well as being surprised by its present deficiencies. It need also to be appreciated that the nature of the invariant relations as also the specific elements of the invariant relations (whether causal or not) will be different according to the difference in the behaviour to be explained. The answer to the question "why John failed in his examination?" may range from (the near-absurd) not having secured the required percentage of marks to (the not so absurd) his great-great-great maternal grandfather having been an idiot, and the range of possibilities is, of course, wide open. The precise specification of a problem is already part of its explanation.

The background has taken more time than I should have liked, although there are still several points which might have been indicated. But I must now proceed to examine the nature of explanatory relations in economics. The background should be quite helpful, and, where unavoidable, I might later introduce points beyond those already covered. I have a certain option of procedure, and I shall opt for proceeding from the more general to the less general propositions in economics. And my appraisal will be no more than suggestive.

I begin with the invisible hand—and perfect competition and rationality. The problem is, how it all works, and the hypothesis—but I had better wait. 'It', obviously, refers to the economy as a whole, and "economy" must already be assumed to be an exchange economy (so because a non-exchange economy will not allow the problem to be raised, or allow it with altogether different terms of reference). Now, if the reference is to the 'whole', it must imbibe the common property of all its elements (constituents or sub-sets), just as water must imbibe the common property of all water between just above freezing, and just below evaporation, point. The quest for such a common property would, I think, lead one to reach up, or down, to instinct, the instinct of survival. One may now postulate rationality, whether in its weak, self interest, or in its strong, maximization (optimization), version; the instinct of survival implies rationality, but not vice-versa. Rationality, then, would appear as the necessary condition for "how it all works." Is rationality the sufficient condition, or the sufficient condition as well? The animal instinct of survival would appear to be both the necessary and sufficient condition: certainly the sufficient condition. The only factor which would occasion the need for a sufficient condition other than rationality itself would be the possibility of certain rigidities emerging in the system. What could the rigidities be, and what could their consequences for the system be? Could one conceive of any rigidities in the animal "economy"? What possibly could make



a qualitative difference to it? With the layman's knowledge of the thing, I find it difficult to imagine the possibility. In the human economy, on the other hand, one might imagine different degrees of efficiency or inefficiency, given that the economy does in fact know what is efficient. It would be neater still if the problem, "how it all works," already imbibes the efficiency attribute, so that the problem really amounts to, "how does it all work as efficiently as it does." Only then can one search for a sufficient condition (or sufficient condition as well) other than rationality. The possibility of rigidities is now also visible: anything that tends to make the system less efficient than it would otherwise be. And, sure enough, one thinks of imperfections in competition. Rationality, then, is the necessary, and perfect competition the sufficient, condition for how it all works (etc.). Can perfect competition be both the necessary and sufficient condition? I dare say, no, if only because I do not see how the economy as a whole (or even how it all works, etc.) can be the equivalent of perfect competition. From one point of view we reached up to the animal world; it is possible also to reach up to the world of Marx: class struggle is the expression of rationality (or the animal instinct) in its attempt to thwart the rather too rigid rigidities. Or, more respectably, to the world of governments. But, more relevantly, are we in the world of causal or, the less dramatic, invariable relations? But I should have to wait, again. If the problem simply were, how it all works, the hypothesis might well be something like, "If there is rationality in it it all works as it does," or, if one wanted to make it look more human, "If people are rational, the economy works as it does," and this should be (except perhaps for the substitution of 'people') all right for any animal economy as well. And it could be turned into a causal relation, e.g., "Rationality makes the economy work as it does." "Perfect competition makes the economy work as efficiently as it does." It is only the next step to see that once perfect competition is assumed to ensure perfect efficiency, we have the hypothesis, "If there are no pressures making for imperfections in competition, the economy works most efficiently"—something parallel to Newton's first law. I suspect already I am (economics is) quite close to the precipice, but let us see. Rationality can no more do with its weak version; the strongest possible is in need, and maximization will do it perfect. Perfect competition has also to be stringently defined, and this can be done. How, now? Well, now, it all depends—whether one can still turn over the precipice or not. And something awfully serious has meanwhile happened that should enhance the possibility of the turn over—(fortunately?) an acrobatic turn over though, as I shall pre-

sently present. What has happened is that perfect competition has now (with those definitional exercises) become both the necessary and sufficient condition for our (revised) problem—and, naturally enough, our most efficiently working economy and perfect competition have become equivalent. As neat a causal relation as one could ask for; but perhaps more than I myself asked for—and a little too embarrassing. The definitional exercise has turned the hypothesis itself definitional. Newton's first law, not capable of being directly verified, can, still, be indirectly verified. Our hypothesis does not need to be verified at all—any involvement with causal or non-causal relations now looks silly. One might, but why should one now bother to, ask whether the strong (est possible) version of rationality was also a definition, or a hypothesis, especially as one already knows the answer (that it is in fact an assumption—and if one persists by asking which discipline's hypothesis does the assumption represent one has the near-sure chance of being considered stupid). The battle between the precipice and method has ended in a phyrrie victory—at the cost of, I would say, the explanatory potential of economics. Certainly, the weak version (the old man Smith's invisible hand) was at once less illuminating and less dark.

I began with what I considered to be the most general proposition in economics, and I have met the fate I have. What shall I take up next? I already have a, rather well-based, premonition. If the proposition I have considered is in fact the most general proposition of (in) economics, I should be surprised if the rest of the science's (?) propositions will be entirely free from its rather powerful tentacles. I shall take a different path—to where I expect weakened tentacles—macro theory, leaving micro theory aside for the moment. I myself have a definite (right or wrong) view on the situation. The whole difficulty arises largely because of treating of human behaviour as the focus of economics instead of treating of it as one of the explanatory variables relevant to the understanding of the behaviour of the economy, which, as I see it, expresses itself through product behaviour. But I must return to where I was. The problem is the same as before, but with an explicitly aggregative hue, the behaviour of the economy as a whole, as expressed in national product: how is it determined both when its behaviour, for ease of analysis, is abstracted from time (macro statics) and when it is allowed to register the full impact of time (macro dynamics). The hypothesis, giving the solution, is basically the same for the timeless behaviour as for the behaviour which takes account of time; except that, as one would expect, the group of behaviour

acting as the explanatory variables is supposed to be unconscious of time in the former case while it is fully alive to the consequences of time in the latter. The variables themselves (ignoring, everybody's mother, nature) are labour force, capital, and technology (and, by one remove, population, saving, and inventiveness respectively); all of which, as already indicated, change quantities (in the case of technology, quality), and acquire rates, to be able to influence the rate of change of national product, in the case where national product is experiencing the impact of time. In the timeless case, the variables carry given quantities (and technology, given quality), and even where changes are admitted, the variables, since they must ignore time, just jump into new quantities all at once, as it were. I am not forgetting the relations; all the variables are related to national product, by first being related among themselves.

There are nuances of course, but they might stay on in the waiting room for a while more. One thing can be stated at once. Causal relations are out. The explanatory variables themselves are, partly, explainable by the behaviour of national product. They both, between them, imbibe, and express, the weaker relation, invariant relations; and, by virtue of (part) reversibility, are quite conveniently expressed through functional relationships. From here on a whole range of (basically method-wise) possibilities open up, and these are in fact quite fully exploited. These, too, can wait; indeed, these are none else but the nuances—more or less. As a matter of detail, perhaps I should mention that behind the explanatory variables, there is something that economics makes use of, without any exception, in all of its explanatory pursuits—the demand-supply construct. The construct in the timeless case comes as aggregate demand and (expectably, rather sleepy) aggregate supply, and in the case where time is of the essence of the matter as income generation (demand side) and capacity creation (supply side), such that the various explanatory variables express themselves through this construct. I personally think all this, as far as it can go, is quite genuinely empirical—in spite of the demand-supply construct. I put it this way because I can hear the danger bells of definitionality which the demand-supply construct is always eager to magnify. And now I must open the doors of the waiting room, to ask in, not, however, the nuances, but, those that had been waiting since before. Enter micro theory.

Actually, our acquaintance with the invisible hand implies our acquaintance with micro theory. It was a case of necessary condition, though. The sufficient condition for our acquaintance with micro theory is to be provided by what I now propose to get in touch with,

price theory. The problem is, what determines prices; and the solution is given by the hypothesis: price is a function of demand and supply. Of course, it is the same very demand-supply construct of whose omnipresence I had spoken a little earlier on. But there is a difference in its presence here; it is as full-blooded as it can ever be. Far from lying behind (as in the macro setting) the real explanatory variables, it is here (in its present, price, setting) quite in the front, pushing whatever real explanatory variables there are themselves behind. Any way, the hypothesis I mentioned just now has two more (sub—) hypotheses for it to be effective, and these are: demand is a negative function of price, and supply is a positive function of price. I need not point out that causal relations are out. And there is, as against what we encountered in the macro case, no such weak stuff as 'part' reversibility; we have here a fully reversible functional relationship. And, naturally the invariant relations. Only, if I might say, the invariant relations are about ghosts. The whole construct, and, since the construct is here *the* theory, the whole theory of pricing, are less than wholly empirical; they are, basically, definitional. I need not here elaborate, especially as my term of reference is the place of causal relations in economics. But the nuances might as well be invited in. The fact is that the nuances as a species have their birth place in micro theory, so I had better continue a while in the same setting.

I had, in the background setting, spoken of sciences being satisfied with sufficient conditions. The sufficient condition in economics expresses itself, in all the various contexts, both in micro and macro, as unique determinateness of equilibrium, such that stability of equilibrium is of supreme significance. To be fair, it must be said at once that economists are no fools (and this is no reflection on what they produce, which, I understand, is not excitingly explanatory, predictive). They quite realise that stability and all that cannot be imposed on their universe of discourse, which, unfortunately (?) is empirical. But once you acquire the mathematical equipment, it is rather difficult to restrain the temptation to make use of it. And, for one thing, the exercise in itself is quite absorbing; for another, it provides the science (and the society) with a standard of reference; and, moreover, if the society could only provide the wherewithal (by way of minutest possible data and making people, institutions and technological laws behave as stability of equilibrium required), the exercise would most certainly have not been in vain. I find that I need not trouble the macro nuances, after all—growth equilibrium and all that; they are, in micro and macro alike, all alike.

But there is still at least one more thing that I should feel

tempted to look into. I have, basically, matters money in mind. I shall, this time, go back to the rather early days of the science. The problem is, what determines the value of money—and the companion (actually the same) problem, what determines the value of all goods and services taken together. The hypothesis I want to consider is, the value of money is a positive function of the quantity of money—indeed, the relation is proportional. Monetary equilibrium itself is related to the prices eventually imbibing the full consequences of any change in the quantity of money. Alternatively, monetary equilibrium is related to the equality between the money rate in the loan market (Wicksell's market rate of interest) and the rate of return on capital in commodity markets (Wicksell's natural rate of interest). The latter, alternate, version as well as the first postulates the same relation, the only difference being that in one case the equilibrium is directly achieved while in the other case it is achieved indirectly; through changes in the rate of interest (after the process starting with, say, an increase in money supply has worked itself out, culminating in proportionate increase in prices or which is the same thing, fall in the value of money, interest rate falls in line with the rate of return on capital, which itself is assumed to have remained unchanged). Here at long last we have a causal relation—and, change in money supply being both a necessary and sufficient condition for its value, change in the value of money (or change in prices) and change in money supply are equivalent; one can be inferred from the other.

Feeling encouraged, I might as well look up a few other of the older hypotheses. I put it this way because the one on the value of money is really one of the older, and the chances, I suspect, are that it is only the older ones that can be expected to be more encouraging. Yes, quite a few of these exhibit causal relations. Let me take up one of the most fundamental—and one in price theory, too. We have there the so-called natural price, cost of production determined real price. It is neater (ignoring his shilly—shallying with plus profits) in Ricardo—labour embodied as the determinant of natural price. And I shall not mention Marx. It is true that these older things had use for the demand—supply construct as well. But the construct was meant solely for the market price, which, as well as the construct, was in any case, a shadow of the natural price; and not even two shadows can turn the theory into something altogether definitional. The situation is different with the modern theory on the problem where one has ghosts rather than shadows—and ghosts, if existential import were irrelevant, can do what shadows cannot. Older macro theories are also genuinely empirical although the relations there are,

by and large, non-causal; they are, essentially, expressed through reversible functional relations. Except perhaps in the more recent (by no means recent enough, though), less old, Schumpeterian hypothesis, where innovation (and innovators) might appear to provide a causal link. Or perhaps it is an intermediate case between explicit causal relations and, the rather general, invariant relations. Keynes' hypothesis, so much more recent (and yet not recent enough), is decidedly non-causal; it is in fact easily amenable to a more or less reversible functionality. His was a timeless case, though; and the focus under-full employment equilibrium. Certainly, not one of the nuances; it was too empirical to be one—and in the bargain it generated policy, had predictive capability. Modern economics (I mean the most recent) has declared most of the older stuff museum pieces—causal relations and all. I could easily present many more of the museum pieces, and the corresponding glittering novelties—nuances and all; but my sixth sense tells me that if I did not restrain the temptation I should be prepared to end up with more of bulk than of substance. And already there is a question, and a moral. The question is, how is it that the really oldest theories have a tendency to go causal—and the most modern to go definitional. The background already answers the question (s). But there is, I think, room for bringing out one implication.

The implication is that if the choice is between a causal relation-based rather weak explanatory potential and a (more general) invariant relation-based equally weak explanatory potential, the question of choice demands to be turned into a question of inquiry into the nature of the invariant relations that have been established. The background anticipates this, too; and even otherwise I have already given the game away. It is more likely than not that the invariant relations (certainly in the most recent stuff, those that are almost wholly absorbed with equilibrium, micro and macro alike—and more so in macro dynamics) are not genuine—not empirical, or not empirical enough. I cannot escape the feeling that modern economics, in displacing causal relations, has landed itself up, by going in for definitional constructs, in the worst of both the worlds. This is truest of the neo-classical micro and the top new macro dynamic formulations—little remains uncovered. The tendency, I should imagine, is, if I were to state it as a principle, particularly marked in micro as against macro, and in dynamic as against static, formulations. Micro dynamic formulations are still in the process of showing up but I expect the eventual shape of things to be quite staggering. Macro dynamic (equilibrium—growth equilibrium) formulations (my principle to be recalled) wholly compensate, through their dynamism, for any handicaps as compared

with the micro ones. So when I now turn back to the question of choice, I find that I would much rather have the older causal relations: I expect them to have (in their own time they certainly seemed to have) more of predictive capability—the power to generate policy—than the definitional uniquely determined stable equilibria. The moral will entail repetition.

## CHAPTER 5

# Value Specification In Economic Theory

By value I mean the definitive attribute of a variable, or of a relation, relevant to the universe of discourse of economics; and the attribute, conceptually, is quantifiable and, possible, measurable. The variable or relation is, of course, empirical. It is possible to 'invent' a purely analytic (non-empirical) variable or relation, in which case, it must be explicitly, and unambiguously, stated, and understood, as such; such an invention might facilitate uncovering the empirical inter-connections, it cannot substitute them. Placing a variable or, especially, a relation already implies hypothesizing; so, of course, does specifying the definitive attribute. All this relates to theory-making in economics. My purpose here is strictly limited: given economic theory, is there anything suspicious or objectionable about the way the theory makes value specification of its variables, and that of the relations among its variables? I should myself think there is, and I propose to devote the following pages very broadly and generally to indicate my misgivings. But I must, first, get quantification, and measurement, out of my way. Measurement, of course, implies quantification, and quantification implies a certain qualitative attribute which, indeed, is what is quantified, measured; and the really interesting distinction relates to qualities that are intensive (such as, hardness, temperature, density), which, being asymmetrical and transitive but non-additive, are measurable in terms of different degrees of the quality in question arranged in a series, and qualities that are extensive (such as, weight, length, area), which are additive. The latter, indeed, is fundamental measurement, which through appropriate numerical laws, allows for intensive qualities to be derivately measured. Much of this has little more than fairy tale relevance here, since economic theory finds it necessary and, in a way, possible to carry on with much less fundamental measurements. It is even doubtful if it has sorted out its qualitative attributes well enough. I have reached the starting point.

Economics has a measure which is all its own. It had, for quite a while, toyed with the idea of evolving a more fundamental measure,



such as, the labour-commanded, but it now seems to have settled at prices. It is a rather unusual sort of a measure inasmuch as prices are themselves measured in terms of money while money (its value, that is) itself is measured in terms of prices; money, of course, is measurable in terms of number as well, but the difficulty is that a billion rupees at one moment may well be less than one rupee at another. The money-measure, in other words, is inconstant. The discipline here is involved with problems which the neat distinction between intensive and extensive qualities cannot very hopefully be invoked to help sort out.

But I need, for a considerably long time, to continue with prices—relative prices. Do prices denote a value specification? If so of what variable or relation? A price is a value specification of the relevant product, where product is defined as anything that enters the process of exchange. But the various different prices (and, the quantities of, products) are all inter-related in their equilibrium values; that is what the general equilibrium system brings out. The fundamental value specification, then, is equilibrium value—the value which binds the whole economic cosmos together. The binding is by no means rigid, or perhaps even a fact except insofar as the system does indeed go on; but the system goes on precisely because there is something in relation to which to go on. How does the discipline make this fundamental value specification?

Assume that  $x_1$  and  $x_2$  are a function of  $p$ , such that there is a certain value (equilibrium value) which disallows  $x_1$  and  $x_2$  permanently to move away from it. To ensure the result let us postulate that  $x_1$  is a negative, while  $x_2$  is a positive, function of  $p$ . Is the result ensured, or shall the negativity and positivity require to be further specified? The answer, I think, is that the result is ensured if all that is needed is whether there will be some equilibrium value of  $p$ —not ensured if value specification for the equilibrium value of  $p$  is sought for. To ensure the latter result let us postulate that maximization (or minimization) of the relevant objective function denotes the required additional specification. This, indeed, will be a special case of the corresponding negativity or positivity; the value of  $x_1$  and of  $x_2$  may or may not be the maximized values. It is possible to treat of the negativity (or positivity) as a purely statistical relation; in which case, the super-imposition of the objective function on the negativity (or positivity) would appear to be redundant. The specifically maximization-oriented negativity or positivity cannot, however, be taken on by any purely statistical relation, unless, of course, maximization itself is statistically “catchable”—which, let us say, it is not, either direc-

tly or, through its deductive consequences, indirectly. And, of course, there is no manner of comprehending the deviations, from equilibrium prices, which actual prices will have been imbibing. This, in short, brings out the nature of the fundamental value specification in economics, where  $x_1$  is demand,  $x_2$  supply, and the not specifically maximization-oriented negativity of demand, and positivity of supply, is self-interest. Self-interest is the weak version of rationality while maximization is its strong version; it is the latter which is to be understood as the operative version for the foregoing. Is the specification empirically meaningful, the procedure acceptable?

I would not say that there is, in principle, anything wrong with the procedure, which might be described as the procedure of postulating an analytical objective function, something that attempts to examine the relevant empirical inter-connections in terms of an ideal analytic model. Unfortunately, the procedure, in this particular case, does not pay off—it blows up, into empirical nothingness. The value specification for the equilibrium resulting from the value specification for the maximisation-oriented demand and supply functions is less than wholly empirical. The ideal remains, but that is poor solace for an empirical science.

Is the alternative, then, to go without value specification altogether? Yes, if the ideal specification is the only specification that the theory needs. But I do not think that the theory cannot do without the ideal specification. After all, it is, functionally (*i.e.* the way it helps in the elaboration of the price theory), an analytic, technical, ideal just as the postulate of perfect competition had for long remained a social (as well as an analytic) ideal: it is so much easier to achieve mathematical elegance thereby. Obviously, the weak version of rationality deprives the discipline of such elegance but the real richness of the discipline depends on empirical rather than analytic content, already convincingly illustrated by taking care of non-perfect competition situations. The ideal specification is certainly preferable, but it is just not available if the relevant empirical inter-connections do not make it available. On the other hand, its non-availability does not at all spell disaster. The theory can do with less; only it will need to be less committal—as committal as the relevant empirical inter-connections permit. The ideal value specification allows the theory today to assert that price, everything remaining equal, is determined by the maximization-oriented demand and supply functions. But even without such an orientation (*i.e.* even with the weak version of rationality), the theory can still say that the price (the value specification for the equilibrium) is determined by

the negatively inclined demand, and positively inclined supply, functions; it all depends how negatively inclined the demand and how positively inclined the supply, functions are in any particular case. In essence, the ideal specification (the strong version) achieves scarcely different—except, of course, for ease of analysis and mathematical elegance. Little lost, I think, considering what material truth allows. The re-formed equilibrium value specification will, in all probability, be different from the ideal one, but that, I think, is all but irrelevant. The only doubt about relevance is as pertains the conceptualization of how the system goes on. I myself am not at all alarmed. The system goes on in relation to the re-formed equilibrium as well as with the ideal one. Only, in the bargain, one's conceptualization is, materially, far more truthful. Whatever consideration may be decisive for retaining the ideal value specification, it certainly does not lie either in the theory's task of determining prices or in the theory's conceptualization of the interrelated elements of the economic cosmos.

What of the other value specifications of economic theory? I shall be severely selective; that would serve my purpose. But I do not think that my impression that economic theory elsewhere has done better in this regard is wrong. Take the aggregate of all products—the national product, which itself implies, denotes, a value specification. The value specification here is in terms of the quantities of individual products multiplied by their respective prices—actual prices; in effect, absolutely free from price theory's ideal value specifications. Curiously, there has been an interim while in the history of the discipline when certain voices attempted to question the value specification national product carries; indeed, to question the very concept of national product and other such macro concepts. That was the heyday of price theory's ideal value specification. Not that it is all over, but the rather pedestrian value specification carried by macro concepts has come to stay—and to be respected. The macro value specification has its own difficulties though; basically related to the inconstancy of the money measure. We have already noted that a billion rupees could well be less than one rupee. Of course, I had imagined a rather extreme case. But the point is that the same phenomenon expresses the difficulty about macro value specifications. The same physical quantity of national product (to continue with the extreme case) which was valued at one rupee at one time could well be valued at a billion rupees at another; the value of money is the exact inverse of the value national product carries, the prices with which product—quantities are multiplied. The way out is the index number; deflating actual prices by a certain chosen year's

actual prices—not altogether a faultless procedure but, under the circumstances, it makes do. Certainly, more sensible than throwing the baby away with the bath water.

But, of course, it is not even the beginning of macro theory—the determination of the behaviour of national product. Fortunately, it is possible here perhaps a little more easily to escape the ideal value specification. There is growth equilibrium, of course. It is, indeed, possible to assign an ideal value specification to growth equilibrium of the type we have met in price theory. But the fact that the ups and downs (fluctuations) in the behaviour of national product are visibly of social concern, macro theory finds it possible to assign a value specification to the behaviour in question as a constant proportional rate. It follows that all the relevant explanatory variables (labour, capital, technology) must themselves carry the same equilibrium value specification: constant proportional rates. The idea simply is to specify a value which represents full-capacity use; in other words, freedom from fluctuations. It will be noted that deviations from the equilibrium rate of growth, unlike the situation in price theory, are measurable. The explanation for this particular difference no doubt lies partly in price theory being essentially static, but only partly, since even in macro statics (as in Keynes), full employment as a term of reference allows for measuring deviations.

But there is something of perhaps more immediate interest. Does macro theory tell as much in its domain as price theory does in its own? And is there fundamental difference in the manner of telling? If macro theory abstracts from relative prices, so does micro theory from the price level. They are thus in this respect quits. But macro theory's special advantage, in this particular context, lies in its being able to avoid a major complication: it can do without having to invoke either the analytic or the social ideal specification. This is the basic difference as regards the manner of telling. As regards what the two theories tell, macro theory would appear, in one crucial respect, to be able to tell much less than what price theory is able to manage. This is best exemplified by (*e.g.* Hicksian) growth equilibrium, which has, unlike (micro) equilibrium price, little built-in power to discipline its explanatory variables to induce them to keep around itself. The whole meaning of equilibrium would appear to have undergone a sea-change. The two kinds of difference are of course not at all unrelated. The built-in power emanates from nowhere but the social ideal specification (which the invisible hand secures from individual self interest), and macro theory cannot eat the cake

and keep it too. Of course, macro equilibrium can be defined in terms of the social ideal, in which case the built-in power can be invoked. But somehow the thing does not seem to click.

Why? Let us look a little more intently at the two sets of explanatory variables in relation to their respective equilibrium values. It is rather a curious muddle, but one might try to waddle through. Some preliminary steps, first. Prices, in economic theory generally, are a phenomenon (a behaviour calling for explanation), a measure (conjointly with money) of value specification—of all value specifications in the discipline, indeed—, and an instrument of (acting as signals for) the competitive (market) mechanism. Now, the market mechanism itself operates through demand and supply which, in turn, as parents, as it were, expect their creation, prices, to do what their granny (the competitive mechanism) expects them to do. It would be quite interesting to go into these filial rights and responsibilities, but all that I need here to note is that heredity is decisive; prices are not only what their parents make them to be, they are as effective, or ineffective, instruments as their granny is effective or otherwise. Prices, to change the metaphor, are servants, and servants, normally, have little discretion. Now, it is possible to conceptualize the whole economic cosmos as an expression of the competitive mechanism, therefore, of demand and supply; and therefore of prices (and the related quantities of products). The only analytic (theoretical) construct, then, is the demand-supply construct. The difficulty with such a conceptualization, and such a construct, is that it breaks down when specifically macro elements are introduced. Surely, it is rather incongruous, to speak of aggregate exchange values. Of course, there is the aggregate of prices, but this is neither a summation nor an average of the exchange values of individual products; it is the value of money in terms of all products taken together, national product; an altogether different stuff, obviously. But the demand-supply construct works; the value of money is generated by basically the same demand-supply construct. More significantly, how about the determination of the behaviour of national product—in terms of the demand-supply construct? There is no doubt that the construct tends to weaken as it travels from price theory proper to macro statics and, more so, to macro dynamics. But I will not go so far as to say that weakening signifies progressive vanishing; the fact is that it is called upon progressively to carry lighter explanatory burden. The real factors keep behind in price theory while they push the construct progressively behind themselves as economic theory enters the macro frontiers. The real factors themselves cannot thwart the

forces of demand and supply through which the competitive mechanism expresses itself. But with macro elements explicitly introduced, the competitive mechanism cannot help find itself hard put to it having to influence such elements rather indirectly. Once again, I would not say that the mechanism has weakened in an essential way—since it is all there—but it is obvious that the immediate response pattern of the micro price context is missing. Actually, it should not be unduly surprising; after all, there is some difference in this regard even in micro theory as between the so-called (I put it this way because my ‘product’ included factors as well) product pricing and factor-pricing. And, of course, I have not yet invoked the fact of competition being less than perfect. The implications do not need to be spelled out.

The explanation, then, is that the macro equilibrium value specification has rather too weak a support from the competitive mechanism to be able to acquire the necessary gravitational pull in relation to the explanatory variables. I need not remind that the Classical conceptualization, working with a robust competitive mechanism, could (except for Malthus) impute the necessary gravitational pull more easily than what the objective situation can allow the present-day economic theory to do. The Classics, moreover, had simultaneously provided for equilibrium value specifications separately for each of the explanatory variables as well: the subsistence wage level, the over-all rate of profits. Also, the Classics had put a much lighter burden even on their price theory demand-supply construct; the relevant equilibrium value specification had been put outside the construct—it was given by their natural price. But this last has implications which I need not here go into.

I should, however, like to end at as hopeful a note as I can make it. There is nothing frustrating about it all—so long as the discipline takes courage to do away with spurious value specifications such as that in its price theory. A science can do no more than what its relevant empirical universe permits. It invites frustration the moment it tries to do more; it may end up with worse. For it is likely in the process actually to do less than it could; but, even if its scientific conscience were not all dead, value judgment and/or too excessive zeal for deductive elegance at the cost of empiricism will be difficult to be avoided altogether. Into this, too, I need not here go,

## CHAPTER 6

# Equilibrium in Economics

I propose here to examine the nature, and theoretic-analytic consequences, of the concept, and technique, of equilibrium as used, and usable, in economics. There are several, alternate, ways to begin, but I would prefer to begin by examining certain relations established in Classical theory with a view to ascertaining if it could provide me with some helpful insights for my main task. I begin with price theory. We have two prices, one basic, the natural price, the other rather subsidiary, the market price. There are two, alternate, ways of viewing the determination of natural price; as the sum of the natural (or, if one wants to avoid unnecessary complications, market) prices of the factors that have been used in the production of the relevant commodity; or, as the (physical) amount of (qualitatively averaged) labour used. Clearly, the former entails a circular mechanism linking all (natural) prices in the same conceptual (and empirical) process; it is endogenously determined. The latter is exogenously determined. The market price is determined, with reference to the relevant natural price, by demand and supply. The reference to natural price is no mere courtsey; demand and supply cannot be powerful enough wholly to bypass the constraint imposed by natural price: market price, while free to depart from natural price, must keep in touch with the dictates of natural price. Lastly, demand and supply themselves have to cope with a built-in constraint of their own: the market price that emerges follows from the relative strength of the relevant demand and supply. I should myself think that one of the elements in the relative strength (or weakness) of supply is derived from the relevant natural price itself, but, in any case, it is the interaction between demand and supply that, given the constraint of natural price, determines market price. Now, why is the (market) price that emerges the price that does emerge? Why, in other words, could it not be different? The answer, probably, is that if it were different, it would not have fully imbibed the relative strength of demand and supply, so that the un-imbibed portion will continue to put pressure until nothing remains unimbibed;

and this means that the price that emerges is the price that must emerge. It is possible to ask why is the relative strength of demand, or of supply, what it is; and if we did that we should find that a similar question would arise at that remove as well, and even beyond, until all prices and quantities are apprehended as assimilated in the circular mechanism I spoke of earlier on. The immediately relevant point for me, however, is that not only are these prices what they obviously are they must be what they are; and these that must be what they are, are of considerable importance to economics as a science. Of such importance, indeed, that economics has a compulsive (scientific) need to go as deeply into it as it can, and since it facilitates working on it, the science gives it a name: equilibrium price. It should not be difficult to appreciate that, unless the attribute (of equilibrium) were specific to price, economics would assign the same appellation (equilibrium this or that; or, this or that equilibrium) to any behaviour imbibing, and exhibiting, that attribute. It could be any name, but this one has the advantage of carrying what the nature of the empirical behaviour in question, in essence, signifies: a tendency to settle at the point the objective situation demands. But the fact that a point (the equilibrium point) exists, and even that there is a tendency to converge with the point (whether we ask the market price to ignore, or to be constrained by, the natural price), does not, of course, at all mean that the point is, in fact, the point which price (or any other similar behaviour) expresses, the price which there is for everybody to see—always or frequently or ever. What, then, if I may say so, is the scientific rationale of the significance of ‘equilibrium’ in economics? I postpone trying to give an answer; and pass on to some other interests of Classical theory. The rate of profit in the various different industries is supposed to tend to converge to a certain given rate; since if this did not happen, the consequent over-and-under-investment in the relevant industries will soon enough (which means, eventually) make it happen. The certain given rate of profit, to which the other rates, converge, is something akin to natural price, the rate of profit in the wage goods sector. Once again, the convergence need not necessarily come about, in fact. In this, as in the price case, the fundamental factor which is ensuring the tendency to equilibrium is competition; indeed, this, too, is, essentially, part of the price case. And, of course, it is through demand and supply that, in this as in the other case, competition expresses itself to ensure the tendency. What if competition, for one reason or another, gets constricted? There are several possibilities—but all through what happens to demand and supply. Both demand and supply may get equally con-



stricted, or one more than the other. The equilibrium point will have changed, and the correspondence (or lack of correspondence) with it will, as the case may be, follow the lead of supply more than that of demand, or vice-versa. In the special case of oligopoly (and I have here slipped beyond Classical theory), where the equilibrium point is a near-non-starter, the very conception of convergence will nearly be without much meaning. But no fears in the so very competitive world of the Classics. I shall now invoke another price—wages. Demand and supply here again; and here again something akin to natural price—the subsistence wage level. And the same ifs and buts. But sure enough the competitive mechanism ensures the tendency to equilibrium. There is, then, monetary equilibrium, which is conceptualized in terms of the quantity theory of money—the counterpart of natural price again. The tendency to equilibrium is either direct or, through the readjustment of the market rate to the natural rate of interest, indirect. Lastly, the behaviour of national product, where, leaving aside the nuances, the crucial thing is accumulation; and accumulation lives on the rate of profit. Hence the possibility of the stationary state; which, however, is quite different from equilibrium. But macro equilibrium was very much there, all the same; that is what the Say's law was about. The ever-equilibrating competitive mechanism was a sufficient guarantee for the tendency towards it. I do not here see anything akin to natural price; no special props are called for. Why? Simply, I think, because all the various props are available to it. If the competitive mechanism works through the demand-supply construct (the price mechanism) and if all the prices, including the price level (the macro counterpart of the relative prices), are relevant to the behaviour of national product, and, lastly, if all the prices tend to their respective equilibria, what else could possibly be required for the national product to tend towards equilibrium?

Can I face up to the question, now—about the scientific rationale? Not quite; but I shall attempt a provisional answer. Competition (picturesquely, the invisible hand) was, howsoever implicitly, an ideal as well as a mechanism. The mechanism first gives the obvious, the prices that are there for everybody to see, and act on. In a limited sense, this is trivial. But the obviousness conceals a logic: while the actual prices are the effective prices, they have to reckon with the competitive pressures not yet imbibed by them which incessantly prompt then towards their respective equilibria. The equilibria themselves are by no means given and fixed, so that the actual prices have an additional reason for not wholly coinciding with the equilibria. But while the actual prices are, or may ever continue to be, disequili-

brum prices they cannot escape the responsibility of tending towards their equilibria. Were this all there is no room for there being an ideal as well. Under the regime of free competition there is no schism between the ideal and the fact. The economist as a citizen assesses what the social ideal could be and he finds that competitive equilibrium coincides with the ideal. But competition is as expressive as it is free, and any limits to its freedom makes all the difference. Self interest may still continue, in the inherent choice making, to be as expressive as ever, but the range of choice—the available alternatives—will have been restricted. And demand and supply, through which competition expresses itself, will have been carrying different values; so prices will have been different. Consequently, the likely gap in relation to the relevant equilibria will have been widened. The tendency towards equilibrium lives in principle but it cannot but be a rather weak tendency. But, more fundamentally, the equilibria themselves which emerge from the relatively unfree competition will be different from those that would have emerged had competition been wholly free. It is here that the ideal springs up as the social term of reference. It is good for the tendency, in relation to the social term of reference, to be strong, to be strong enough to keep in close touch with its relevant equilibrium, and good for the relevant equilibria themselves to be what they could have been had competition been wholly free. After all, the equilibrium is as meaningful as its pull—gravitation—is effective. A constricted competition generates, in effect, another pull; and the more effective the alternate pull, the more notional becomes the ‘ideal’ equilibrium. But the ideal equilibrium must at any cost be retained as the basic term of reference because it is the society’s term of reference. In, even implicitly, allowing the ideal equilibrium to continue as the basic term of reference with the society’s ideal turned on to it, economics as a science makes an accommodation which is by no means altogether dishonest. But I do not see how equilibrium could be as significant as it is without the ideal element. Equilibrium does not answer all the various questions it allows to be raised, but, but for comprehending equilibrium, certain types of questions will not have been raised in the first place. But I must now return to my main thread. I leave Classical theory, for the moment, behind.

There is little to add on the price problem proper—despite revolutions and general equilibrium systems. I am left with macro theory proper; the behaviour of national product. Keynes, in destroying the Say’s law, took away the automatic macro equilibrium. But the demand-supply construct is in tact. In the nature of the thing that

was of immediate interest to him, supply is quite sleepy a partner in his (demand-supply) construct. Actually, Keynes' demand-supply construct, while analytically as meaningful as the construct is in the price problem, is more of a scaffolding (an analytic scaffolding) than it is in the price problem proper, or, to put it differently, less of an empirical (theoretical) conceptualization in its own right. Surely, demand and supply, in the price context, would appear to be acting and reacting with reference to what they are about (price) directly while the real forces they represent, and express, lie behind, by one remove or more. In Keynes, on the other hand, they represent a category, and represent, with reference to the behaviour in question, the real factors indirectly; the real forces are in direct confrontation and demand and supply themselves rather behind, as it were. Now, if what I have said is true (as well as correct), Keynes' real factors must have some way of expressing themselves with reference to the behaviour in question. To continue to see as I see it, the way they do it is through functional relations among themselves with reference, of course, to the behaviour in question. What of equilibrium? It is there—in a way. In a way, because, if I may say so, it is qualitatively different from the normal equilibrium, price equilibrium. We have already seen that Classical macro theory has its Say's law equilibrium. But a see-saw change has occurred: competition so much less than free. The invisible hand has lost its celebrated powress. What has happened in Keynes is that equilibrium, in effect, has been superimposed—in terms of full employment. I know of the under-full employment equilibrium, but I am speaking of the term of reference, and full employment it is. It is a superimposition because it is external, not, as in equilibrium price, built-in within the basic empirical as well as analytic process-mechanism. The ideal which was a prop to the logic of equilibrium is now, if equilibrium has to be retained, almost the sole ingredient. Competition is by no means conspicuous by its absence, but full employment flowing from the interest, logic, of the process can no longer be considered even as a near-truth. Keynes did so much in fact to disabuse one of the illusions. There are no compulsive tendencies. After all, the tendencies are difficult to conceive of in the absence of a genuine equilibrium; they are two facets of the same (one) reality: tendencies appear only if they and the point emanate, and inherently belong to, the same confluence of forces. The point may be borrowed, but it will not stick; it cannot generate tendencies—and tendencies cannot be borrowed. Full employment does for the notion of equilibrium, but it is beyond it to generate tendencies. Tendencies either exist or they do not; it is a wholly empirical question—and a matter of

inter-connections among the relevant matters of facts, the facts which, in fact (in the world of reality), conjoin as explanatory variables, with the behaviour in question. It is the business of theory, genuinely empirical theory, to discover the inter-linkage. The nearer the conceptualization is to the empirical inter-connections the truer the theory. The fact is that full employment is a social ideal, and ideals are not to be discerned in the empirical inter-connections; they would simply not be there. What is the difference? I had spoken of the scientific rationale of the significance of (price) equilibrium also as being related to an ideal. But there is a difference. The ideal in the price case was perfect competition; here it is full employment. Unless one were to say that one implied the other, or even that perfect competition implied full employment, the two are different. But the difference that I want to bring out is that insofar as price had to reflect (imbibe) the relative strength of demand and supply, price *ipso facto* reflected competition as well—how weakly or strongly is a different matter altogether. The ideal has a built-in empirical (theoretical) as well as logical (analytic) process-mechanism to get expressed, and that is what is important—and relevant. In Keynes, on the other hand, what could possibly inherently imbibe and express full employment? I do not see there is any such thing there.

Am I making reflections on Keynes' theory as a theory? I could not possibly be doing so if only because I have learnt to judge a theory by its predictive capability, by its capacity to generate policy; and I understand that Keynes has done quite well on this count. It is, however, not Keynes's theory as a theory—I mean the whole construction—which has given the predictive capability. It is just one element of it, an element which has no doubt been exceedingly well integrated with the theory as a whole, but an element, all the same, that can, given the chance, stand all on its own. I am of course referring to the consumption function, the multiplier and all that. And I think the element is sufficient, as a theoretical construct (I would consider it to be a theory in itself—in the sense of its having discovered an empirical inter-connection), to answer the limited (though by no means easy) question he had posed. (It is here beside the point if it was his or Kahn's theory.)

Before proceeding further I should like to ask myself what, pray, is the predictive capability of price theory—any price theory? With all that genuine equilibrium, too? Practically none, I should say. Am I contradicting myself—am I not contradicting myself? No, for the former; yes, for the latter. I shall explain; I shall explain by, first, suggesting that genuineness does not deserve to be over burdened,

and, secondly, asserting that it is, paradoxically, no fault of that equilibrium that it has little predictive capability. The explanation really is that equilibrium price simply cannot be 'caught'—and not only because it is elusive something. It cannot be caught because it cannot be caught. Not even through the labour-embodied determined natural price. And I cannot even begin to ask, myself or others, whose fault could it possibly be. On the other hand, I can contradict myself—by saying that equilibrium price has unmatched predictive capability; through it, price theory generates policy more profoundly than any economic theory ever can. It shows (or perhaps, given its, one may call, large assumptions, it is made to show) that the least bothersome, the least costly, and the most efficient allocation of a community's resources can be achieved by competition, the freer the better. The theory cannot tell what equilibrium prices (and equilibrium quantities), in fact, are, so no body can tell how far have actual prices (and quantities) deviated from equilibrium prices (and quantities), but if competition is, in fact, not free, it can be assumed that there are deviations; and the less free it is, the greater must be the deviations. And it is not impossible to have some broad notion of the extent of restriction on competition. Of course, it is not exactly as simple as this, since competition has an almost built-in predisposition to get less free if there are appreciable differences in the economic power at command with the various economic agents, so that competition would appear to demand more or less equal distribution of income. And distribution of income, is, in most cases, a historical fact; even what happens today becomes history for tomorrow. The theory, thus, has its own limits, but it goes as far as it can, and it goes quite far.

It is a different matter that, appropriately interpreted, it is really less (or more) than theory inasmuch as the 'ideal' part of the theory is thinly based on the relevant empirical inter-connections. I am not at all suggesting that it has no empirical base whatsoever; but the rationality assumption acquires a progressively doubtful empirical connotation as it travels from the broad and general Smithian self-interest to its strong (strongest possible) maximization version. But this is a matter which had here better be bypassed. At any rate, my having contradicted myself has been a peculiar sort of being contradicted. One did not, after all, need the whole (and these days it is quite elaborate) theory of pricing, with equilibrium and all that, to be able to get the policy it does generate generated. The Classics had said, in essence, as much, and while they did not show it as clearly as we can do now, considering what in fact we get by way of

policy today, the effort, though highly praiseworthy, has, proportionately, been rather excessive.

It is understood that in talking of Keynes I had been talking of the time-less behaviour of national product; even, essentially, one aspect of it. Keynes was wise, of course, since it would have been silly to try to grasp inflation as well as depression in the time-less setting. Actually, neither can be adequately grasped unless one allowed time to have free go with national product. Keynes provided the basic concepts and techniques of analysis though, and we have done our best to refine, and elaborate on, these; as, indeed, Keynes had done with (with or without the Say's law) the Classics, including Malthus, and Marx. The basic thing of course is conceptualization of the empirical process one wants to understand, and Harrod and Domar provided that, too; and they, too, derived all that they could from the same others as well as from Keynes himself. So we now have macro dynamics (which only with a little terpidation of heart could one call the theory of economic growth); even that approved, and chiselled, by Hicks. This last, indeed, is the crux of the whole matter of macro dynamics; or, so one is supposed to view the matter. The fact is that it is all equilibrium now; neat. It carries a qualification, since it is called growth equilibrium, but that is because of unavoidable necessity—to distinguish it from other types of equilibrium. Naturally, then, equilibrium is the beginning and the end of the matter of the behaviour of national product through time. It suits me well enough, since that is precisely what I am after. What is it? It is a superimposition, just as it is in Keynes. The superimposition in growth equilibrium is steady-state growth—clearly a much more comprehensive superimposition than Keynes'. The impression of similarity is not altogether an illusion since steady-state also is full capacity use of all possible resources; but Keynes was dealing with the same very resources in their sleep while these are quite awake in growth equilibrium. Only they are, for the sake of equilibrium, allowed to remain awake in a predetermined manner, the manner that equilibrium dictates. Of course, there is no empirical counterpart to this; and, of course, a theory has to be guided by just that. But the point is that the equilibrium does not ensue from theorizing; it is, as with Keynes (and perhaps more nakedly), imposed on the theory. And we have already seen that if the equilibrium (point) is not an inherent part of the theory, there can be no inherent tendency towards it. There is nothing wrong with this or the Keynesian superimposition, both are social ideals. But they are not part of any genuine (empirical) theory; and if one forces growth equilibrium to carry the

badge of empirical theory (and no theory in an empirical science can be anything but an empirical theory—so much so that were it not to emphasize the point I would not have considered it decent to keep on qualifying theory by empirical), let one not except it to abound in predictive capability. The demand to generate policy, to predict, should instead be addressed to the same sources that made the superimposition, and (do they really ?) expect tendencies towards the superimposed equilibrium. Growth equilibrium is too much of a short cut for grasping the behaviour of national product over time to be of much help to deliver the society's ideal. The society will need to continue to try on its own to achieve the ideal; growth equilibrium of the type I have been talking about can provide little else than what even an intelligent layman of the society knows, or can quickly get to know: (just as the same intelligent layman can, in micro economics, get to know what, given the concepts, is the equilibrium of the consumer or of the producer) that steady-state may well be defined in the way growth equilibrium defines it, and that the relevant resources should be behaving (to ensure growth equilibrium) in the way growth equilibrium prescribes. Keynes gave the best solution for depression that we uptodate have. I should be most pleasantly surprised if something could be provided by growth equilibrium by way of a solution to the problem of inflation—let alone a "simpler" solution encompassing depression as well as inflation. A much more difficult task awaits society before it can learn to ensure growth without too uncomfortable inflation and depression. (And I am not mentioning what I should think is the real problem, growth itself.) Nor has economics, if it is interested, a much less difficult task in grasping the relevant empirical processes. Imposed equilibria have no way of grasping empirical laws of behaviour. This is the business of theory, theory proper. The relevant question for a theory (of economic growth) is not how the (explanatory) variables should behave. The question rather is how, in fact, they behave. Man can improve the breed, of carrots or horses, or control the inanimate nature, not by wishing or wanting them to behave the way he would like them to, but, first, by grasping their own laws of behaviour, which are independent of man. Products, including national product, have also their own laws of behaviour, and equally well independent of man, the fact of their all belonging to, and even being created by man, notwithstanding.

The nearest that I find in literature (and this is in Hicks' conceptualization of equilibrium in his *Capital And Growth*) to what I have emphasized is the 'problem' (?) of equilibrium in welfare economics in that the equilibrium assumption is included in the way the theory

is set up. The game is up, and no artificial differentiation as pertains the case of positive theory (and Hicks tries such a lot) would demolish the implication. There is, however, one consideration which might help the positive equilibrium escape the inherent attribute of the welfare (my 'ideal') equilibrium. Positive theory considers itself entitled to defining its equilibrium (all varieties of equilibrium) in terms of the most preferable (preferred) of the available alternatives (it is a different matter that the available alternatives may be confined to being treated with electrocution and the rope). It is, in fact, this that lends the demand-supply construct the analytic power it possesses—in all walks of life in the universe of economics. A construct through which competition expresses itself. It is this fact of preferences that makes the ideal in the case of equilibrium prices less blatantly superimposed an attribute than in the case of macro equilibrium; one may object to maximization but one cannot possibly ignore self interest altogether, which is what the preferences express. But do (can) preferences have the same manner of effective expression in macro as in micro? I myself think they do not, cannot. The explanation is that the aggregate entities, savings, investment, and the rest of it are bundles of conflicting preferences ultimately showing up as single entities; they are bound to have lost their fighting (competitive) capabilities when they conjoin with one another, and, functionally related, together with national product. After all, it is not for nothing that the Walrasian general equilibrium system cannot adequately imbibe the functioning, and interactions, of the really aggregative entities. It is beyond its internal logic, and so, fundamentally, because the preference system which guides the equilibrating mechanism cannot but fail meaningfully to express itself in relation to the aggregates. Competition can, does, still remain active. But it will need something more than the diffused, essentially micro-expressive, preferences. This is, of course, in addition to what has meanwhile happened to competition. Although, I myself think, it is an exceedingly significant question how is it that while the Say's law has been effectively destroyed micro equilibrium continues basically to be what it has always been—and, with competition so much constructed, with little justification. It may well be that the science needs to be quite outspoken about the ideal element of the price equilibrium. But it may not be the case; competition is, after all, less expressive with the aggregates. The Classics escaped the problem of macro equilibrium partly through non-comprehension and partly through the Say's law. What they had as macro equilibrium is not totally unsustainable. But they had a way out; certainly, they indicated the way out. It will be recalled that even in their price hypo-



thesis, they had a natural price to buttress the demand-supply (market price) construct. They had the subsistence wage level for the behaviour of wages—and labour force. And they had a given rate of profit for the economy as a whole to which the other rates of profit tended to converge. Of course, the Classics' conceptualization of non-product pricing is not all that micro; but so much the better for what I am after. It only means that their way out is easily transferable to macro equilibrium proper. The way out, of course, is to postulate something "external" to, and yet (as an explanatory variable) inter-connected with, the behaviour to be explained; and this 'external' something must be, neither definitional nor purely analytic, nor, indeed, super-imposed, but, reflective of the relevant empirical interconnections. Mere definition either of growth equilibrium or of the conditions of growth equilibrium will not do for explanation, and, therefore, for equilibrium—real explanation, real equilibrium. The search for existence, for determinateness, for stability etc. is, as appears to be the case with the modern (-most) conceptualization of growth equilibrium, not a mathematical, but an empirical search. Schumpeter's view that "if a system or model that correctly expresses fundamental features of the capitalist society contains contradictory equations, this would be proof of inherent hitches in the capitalist system—proof of real, instead of imaginary, 'contradiction' of capitalism" (*History of Economic Analysis* p. 971 f. 17), while being obvious, even commonplace, can be awfully misleading. In any deductive argument, the conclusion must necessarily be true if the premises are true but the whole burden is on the premises, in fact, being true—which is a question not, of course, of logic (mathematics) but of material truth. I have no objection even to wholesale mathematical constructs so long as no impression whatever is carried, or conveyed, that there is even by implication any material-truthwise connotation. In the present context, let equilibrium be defined and its conditions mathematically elaborated; but, then, such equilibrium must be treated as a technique rather than an empirical concept, and the exercise purely analytic rather than hypothetical. The technique, and the exercise, may not be purposeless, but they may be so. It all depends on whether one pats oneself at one's back or simply makes use of it for the real job of the scientist, discovering the empirical interconnections relevant to the behaviour to be explained. It is equally easy to be misled by Einstein's (again, obvious) view that it is theory that decides what matters of facts are to be looked for. After all, looking for matters of facts is looking for evidence, to be able to ascertain whether a theory was true or false. But, then, this demands looking into facts rather than

justifies taking the theory for granted as true. And, in any case, how does one come to have a theory (hyphothesis) in the first place? Surely, through one's comprehension of the, crude, matters of facts and their inter-connections. Conceptualization, empirical conceptualization, is of the essence of the basic business of scientific endeavour. The Classical's had it in abundance; the growth equilibrium I have been talking about is, I think, severely lacking in it.

I was following tradition when I earlier on spoke of near-absence of equilibrium in the oligopolistic situation, but I have a different appreciation from tradition of the matter. The context is pricing, and pricing as we have noted, has much less blatant superimposition of an ideal than the macro problem; the preference system is in its home ground. But already competition is an emaciated force, which can only express itself through demand, or supply, or both being emaciated. The obvious aspect of equilibrium (prices are what they are—for everybody to see and act on—in oligopoly as elsewhere alike) cannot, definitionally, present any problem. It is the 'ideal' aspect alone that can remain hanging in the air. The particular way competition has been emaciated in oligopoly simply does not allow the preference system to strike a balance, the 'ideal' aspect remains unexpressed; or, more correctly, it is, definitionally not expressible. Oligopoly pricing exemplifies a subtle clash between empiricism and mathematics (logic). The particular (emaciated) competition is an empirical conceptualization, something that is not a matter of logic, and the conceptualization has already put logic in its place: logic cannot change the conceptualization and it cannot force the equilibrium to exist. And when the different helpful assumptions are supplied, to induce the equilibrium to exist, and to be determinate etc., it is the economist who is altering the conceptualization—and he must have the last word. Logic can certainly indicate the range of possibilities, but it is the economist who has to decide what particular possibility was acceptable, if at all; and he can decide only with reference to the relevant empirical inter-connections. I know of suggestions to cut the cloth according to the coat, even if the cloth was a wee bit too short (Hicks has a considerable lot of such suggestions); this represents the case of mathematics dictating economics—not its proper role.

Macro equilibrium must be a superimposed ideal—the only difference between welfare and positive economics is that it is definitional in welfare, does not need to be superimposed. Unless, that is, we go by the Classical way out, and discover an 'external' term of reference of the kind I have indicated. I repeat that this cannot be discovered even through the most intricate of mathematical exercises

on the definition either or both, of growth equilibrium or of its conditions. In saying what I have been saying above, I may have been barking up the wrong tree altogether, but there is just a thin possibility that the 'pure' (for me, essentially non-empirical) growth equilibrium theorists might have developed some affection for a similar tree. And while my bark has very likely the prospect of being heard by myself alone, our science is so very receptive to theirs.

## CHAPTER 7

# The Price-Specificity Illusion of the Demand-Supply Construct

The view here elaborated, basically an effervescence of the Classical (minus Malthusian plus Marxian) appreciation of the theoretic-analytic import of the demand-supply construct, is, as the starting point, due to Schumpeter (rather in the nature of an obiter dictum and certainly a not very reasoned judgement), viz. that the construct is not, and cannot be construed as, a theory of value but rather a system of analysis, an analytic apparatus. I shall, going beyond and not quite in conformity with Schumpeter, try to show that insofar as it illumines the behaviour of the price phenomenon, the construct simply exemplifies a special case (application) of its general analytic ascription, and to consider it to be specifically a price-construct is an illusion, an illusion attributable to usage and Marshall.

The Classical position on the construct can be interpreted tersely, although by no means in-controversially. The construct simply translates, in the idiom of science, an obvious, common-sense, even laymanish, appreciation of what happens in everyday experience, and (if one chooses to be fussy enough to ask why) generally supported by one's introspection, and (if fussier still) by something recognizable as self-interest which, in turn, buttresses as well as it is buttressed by a spirit and milieu of competitiveness. The two constituents of the construct, demand and supply, are vehicles, not prime-movers. And they have a rather constricted jurisdiction (within which, however, they can play havoc with each other and with what their play is about, the market price) fundamentally determined by their relevant natural price, a price which is, if one has to have such a notion, real equilibrium price (real=empirical=non-definitional=non trivial). The construct, thus, would appear to have a price-specificity; a market price—specificity, that is. But, apparently only so far as, to use a neo-classical term, product pricing is concerned. So because we have from here on classical positions, not the Classical position—because of differences in the basic components of the natural price,

and because, in factor pricing, which tasks on a macro badge, the natural-market price dichotomy becomes either irrelevant or unspecified. In Ricardo, for instance, where the class shares have a more or less self-contained schemata of determination, rent is a consequence rather than a cause of price, and wages and profits are in an incessant tug of war, with population growth, accumulation and technological change and their inter-relations. And in Smith one might read the dichotomy as being applicable to the factor-share level as well; or, one might not. It would certainly be safer to aver that the construct would appear, in effect, to have a product (market) price-specificity.

The neo-classical demand-supply construct is vastly more powerful. It has no constrictors like the Classical natural price (the dichotomy vanishing, there is no other price but the demand-supply determined price), has equal command over all, factor as well as product, prices, and, for all practical purpose, its two constituents are no mere vehicles but prime-movers. The transformation is radical and complete, but also rather transparent. What has happened is that the components of the Classical natural price, in its cost of production version, has been taken over by the neo-classical construct and thrown behind its supply constituent. The constrictors, in effect, are back, in a different garb though. This is how the dichotomy has vanished; the transformation has resulted from assimilation.

But a qualitative change has already taken place. The neo-classical equilibrium price is unreal, definitional—a phenomenon not of the empirical world, but a product of the world of the construct. The Classical price, even the market price, as far as it goes, is real; only it can be said to be less real than the more robust reality embodied by the natural price. The same holds for the market price equilibrium relative to the natural price equilibrium. To put it differently, the market equilibrium price is the shadow of the natural equilibrium price; and shadows, obviously, are, in their own way, quite real, certainly not definitional. The classical demand-supply construct has also so much less burden to carry, and its burden, too, is genuine, real. The neo-classical construct, on the other hand, apparently carries a considerably lot more burden, but the burden is logical (mathematical), and is easily carried. Except, that is, when the construct nearly kills itself—when, as in oligopoly, it cannot help meeting with an empirical problem; when mathematics fails to deliver the goods. (Thank God, economics is not mathematics). Already, in fact, the construct is one-legged (one-armed ?)—right from the time perfect competition is left behind and the journey across the non-perfect competition frontier begins: the supply curve vanishes; becomes, indeed,

a contradiction in terms. In any case, the neo-classical construct would also appear to have a price-specificity; an unrestricted, general, price-specificity instead of the, apparently, partial (product market) price-specificity of the Classical construct. However, our verdict in either case is clearly premature: we have yet to see if the construct has a role outside the price problem.

We have also yet to examine the theoretic character of the demand-supply construct, and this may well be taken care of first. We have said that, although the Classical natural price is more robustly real than the market price, the Classical demand-supply construct on market price is all the same real whereas the neo-classical construct is definitional. This needs to be further examined. The fact, as already implied in our shadow analogy, is that the Classical market price has, as a theoretic formulation, no existence independently of the natural price. So, although real, the classical demand-supply construct is best understood, not as a theory properly so-called but, as, after all, an analytic construct or model. The neo-classical construct is also an analytic construct or model, but with a difference. It does not even have a shadow-like reality. The difference, basically, relates to the presence or absence of an external term of reference, such as the natural price is for the Classical construct on market price. More fundamentally, the price problem itself can be said to be explainable only in terms of something outside the system, as it were; outside the analytic construct on price, that is. The 'something' outside the system in Smith is cost (of production), in Ricardo slightly different from Smith with emphasis, on labour-embodied and in Marx the Ricardian emphasis, further reinforced, is the sole element. In the neo-classical construct, there is no such 'outside' element and, the way the equilibrium is postulated, virtually no need for it. Systematization through reversible functional relationships, true circularity, is what sciences try to achieve, and it might, on this count, appear that the neo-classical construct has merit, inasmuch as the triangular functional relationship exhibited by the demand-supply-price construct can be said to imbibe this property. But true circularity is not circularity in reasoning. A circularity in reasoning might, indeed, be detected in Smith's cost-of-production-determined natural price as well inasmuch as the cost components must themselves be taken to be determined by their own relevant cost of production. But even if this were so, the demand-supply construct in Smith, for the reason that it has the natural price as its, external, term of reference, escapes this sort of circularity. The neo-classical construct, having the advantage neither of an external term of reference nor of an 'outside' element, is in the

worst of both the worlds.

We may now return to the question of price-specificity of the construct, and see if the construct has a role outside the price problem. Now, since prices express the basic element of the process, and mechanism, of exchange, we may well begin by comprehending what exactly does exchange denote. It may simply mean a process of give and take for a consideration, and the terms at which this takes place imply a ratio, which when expressed through money takes on the badge of price. But it also implies a process of allocation of resources of all varieties, in which prices, acting as signalling devices, determine the pattern of allocation as well being themselves determined through the process. In other words, one may treat of exchange and prices as a particular aspect of the functioning of the economy as a whole or one might conceive of it in a manner that it hardly leaves anything conceivable out, making exchange (together with prices) squarely co-terminus with the functioning of the economy as a whole, something that the Walrasian general equilibrium system exhibits. Clearly, it is only in relation to the restrictive conceptualization of exchange that the question of price-specificity of the demand-supply construct could have any meaning. In both the cases, exchange is equated with or, at any rate, made expressive through prices; the sole difference being that the latter conceptualization does not leave anything out, beyond prices, where the demand-supply construct could possibly have a role. The difficulty with the former conceptualization, on the other hand, is that it demands that the functioning of the economy be conceived of in a manner that part of it is exchange-based, the other part having little to do with exchange, as it were; something that cannot possibly be gulped down. But, of course, there is still another way of conceptualizing exchange; basically the latter, but taking account of explicitly aggregative (macro) entities as well, something that the Walrasian system cannot possibly imbibe. Exchange would, of course, still be co-terminus with the functioning of the economy as a whole, but it would then have become a multi-dimensional process, making for a qualitative or vertical assimilation of the various prices and quantities, representing, in effect, the transition to concepts such as national product, savings, investment and so on, instead of the quantitative or horizontal assimilation that the Walrasian system can legitimately be taken to be capable of.

But if exchange, according to the last conceptualization, is, with all the implications of its multi-dimensionality, to be co-terminus with the economy as a whole, prices, being a necessary concomitant of exchange, must likewise be all-pervading. How, however, to compre-

hend the qualitative, vertical, assimilation of prices corresponding to the similar assimilation of quantities? Now, we know that one such assimilation of prices expresses the relation between money and the aggregate of quantities, the macro counterpart of the relative prices, known as the price level or the general (aggregate) level of prices, (when, unlike the Smithian case where the measure is a price-independent entity, labour-commanded, the measure is the money-measure, the aggregate smacks of circularity of reasoning; but this is a different matter altogether.) But, surely, there is also the concept (and the question of explanation of the behaviour) of national product, and the price level is not the only thing to be taken account of there; there are also the no less immediately relevant concepts of savings, investment and the like. But how to comprehend their behaviour in terms of prices, and prices alone? Of course, these macro, as well as any micro, elements have prices, no matter how these prices are designated. After all, it would be inelegant to speak of the price of money (although we do speak of its value). For the same reason we speak of wages, interest, and profit, although, the economist knows, these are all prices, in the macro as well as in the micro context, exactly as product prices are prices; the exchange process can make no distinction between one type of prices and another. So, after all, it is little else but usage, society's usage, that would appear basically to impart an almost special connotation to product prices as distinguished from non-product (whether micro or macro) prices. It is, more immediately relevant to the point we intend to establish, usage again which imparts the special connotation to demand and supply in the micro price context as distinguished from that in the macro context, whether it relates to the macro counterpart of relative prices or to the macro entities such as national product, savings, investment and the like. By the same token, an artificial connotation, or specificity, attaches to the demand-supply construct on pricing; pricing has already been specially connoted, together with its relation with demand and supply, which themselves are already specially connoted, too. Linguistically, thus, the price-specificity of the demand-supply construct results from a double specificity—that of prices (connoting, in effect, non-macro prices) and of demand and supply (connoting, in effect, non-macro demand and supply).

We have already gone beyond Schumpeter. For he himself considers use of demand and supply in the macro context as almost unfortunate. Clearly, he is no less a victim of usage, of the artificial special connotation attached to demand and supply, and to prices. The position, then, is that, in the Classical and neo-classical setting



alike, the demand-supply construct has a price-specificity which is, fundamentally, a reflection, not of reality but, of society's usage. Economics cannot both consider exchange as being co-terminus with the functioning of the economy as a whole and, at the same time, restrict the conceptualization of demand and supply to the understanding of the price phenomenon, and, consequently, make the demand-supply construct have the price-specificity indicated above. The demand-supply-price construct has no existence totally independent of the conceptualization of demand and supply meant to comprehend the process, and logic, of exchange and, therefore, of the economy as a whole. This wider conceptualization of demand and supply (and, by implication, every construct, including the demand-supply-price construct, that explicitly uses demand and supply as analytic concepts) has to be treated, not as an empirical, hypothesis-based, theory but, as a system of analysis, an analytic relation, a technique which economics uses in a variety of ways and in a variety of contexts, not, excluding the macro context. It is, thus, an expression of usefulness and scarcity in the (neo-classical) theory of pricing, as the relation between aggregate demand and supply (Schumpeter won't have it) in macro statics such as that of Keynes, and as an expression of income generation and capacity creation in the theory of economic growth (macro dynamics) such as that of Domar's. And, of course, in the macro aspect of value; which is, basically, taken care of by the demand-supply-price construct itself. The demand-supply schemata is a vehicle usable by any genuine customers.

It is perhaps significant that the relevant empirical forces in the theory (theories) of macro dynamics are 'open', in that they are explicitly stated at the level of the explanatory relationships itself, reducing the demand-supply schemata to something little more than notional. They are, I think, less open in macro statics; to that extent the demand-supply schemata (of course, in its aggregative version) is more visible, less notional, than in macro dynamics. And they are the least (scarcely) open in the neo-classical micro theory; the analysis is explicitly in terms of the demand-supply schemata, which is in the forefront, while the relevant empirical forces are introduced in the course of elaborating the price construct. It might appear that given the difference in the type of behaviour to be explained (prices, and level, and rate of change, of national product), it is only natural that there should be a difference in the degree of openness indicated above. But this would imply that demand and supply are more directly explanatory variables in the price problem than in the macro static and, especially, the macro dynamic problem. Of course, it all

depends on what the empirical inter-connections, in fact, are. And these, the empirical inter-connections, are certainly in no way indicative of the apparent specificity of the demand-supply schemata to the price phenomenon. I would myself venture the opinion that the supposed specificity is an illusion, an illusion created by little else but usage—and Marshall. The demand-supply-price construct is, after all, already almost a folk construct, something that cannot be said of the aggregative version in the macro static, and much less in the macro dynamic, setting. I have already examined the impact of usage. Let me now take up Marshall.

The Marshallian synthesis, in terms of the scissors analogy and the period analysis, has also had a role in creating the specificity illusion. But it was a synthesis of elements which cannot legitimately be synthesized. In the event, it cannot escape being treated as a double confusion; a confusion between real and notional (schematic), and between natural price and market price, on the one hand, and period-relative prices, on the other. Marshall, in fact, went beyond the basic neo-classical terms of reference. It is significant that while the distinction between natural and market prices already disappears even in their conceptualization, the originators of neo-classicism stuck on to real factors; it is, in the context, not important that they opted for utility in place of cost (of production) or, by a further remove, labour. Of course, the demand-supply apparatus, through which the utility base is allowed to express itself, is very much there. In Marshall, not only does the natural-market price dichotomy disappear, utility as well as cost remains in anything but their own blood and bones; the demand-supply apparatus, now as theory, reigns supreme. And his period analysis, a mix-up of method and content, and the Classical natural-market prices are, of course, birds of different feathers altogether. The Classical natural price is not time-relative; nor is the Classical market price. The Marshallian period analysis is naturally so very time-relative. The demand-supply schemata used by the Classics for their market price is little more than a convenient way of expressing forces making for deviations from natural price. In Marshall, however, the schemata dissolves almost everything but itself—and equilibrium prices. The latter would, indeed, indicate a third confusion: between real equilibrium price (the natural price) and the definitional equilibrium price (what the demand-supply schemata generates). All in all, then, the demand-supply construct, far from having a genuine price-specificity, is, irrespective of the setting, the Classical or the neo-classical, actually a special case of the fundamental analytic (not theoretic) conceptualization of demand and

supply, which, in turn, is a basic concomitant of the economist's conceptualization of the process, and logic, of exchange that is co-terminus with the functioning of the economy as a whole.

## CHAPTER 8

# The Possibility of a Positive Micro Economics

It is only fair that I begin by clarifying why I do not consider present-day micro economics to be positive. I shall do this by trying to show that it is, at best, pseudo-empirical, that it is, basically, deductive, and that, insofar as it is not wholly non-empirical or deductive, it is value (welfare) oriented; also that much of the explanation for why it is so is to be found in human behaviour having been made the focus of attention. I can be brief.

The empirical behaviour which micro theory puts the main burden on is rationality, defined as the human propensity to optimize, as indicated by the most preferred of available alternatives; in quantitative terms, the maximum, or minimum, possible. The difficulty in "catching" what optimization in a particular situation in fact is has a definitional solution: that which is preferred is itself the optimum. Only, the procedure generates another difficulty. Why call it optimum, or, maximization (minimization), then? Begging the question has the habit of ending up in endless circularity of reasoning. Definitions cannot answer empirical questions. Micro economics cannot escape having to take itself at its own words. Rationality is an empirical behaviour, and if one uses it for deriving implications from out of it, one has the responsibility to answer to oneself whether in fact there is this stuff—and in the precise sense in which it is used for implicative purposes. Is it, in fact, there? Micro theory treats it as an assumption. Fair enough, but what would it invoke if asked to produce rationality's credentials? If, as a non-mathematician, I were using difference equations, I will in all conscience turn the person daring me on the validity of the procedure over to the pure mathematician. And, I suppose, I shall have some similar way out if my elevenish son asked me why his oranges did not turn into what his mother called sewing needles. An assumption, in the last analysis, must belong—somewhere; it must be somebody's hypothesis. Now, I know of no science whose hypothesis it is, and I know that economics makes not an inconsiderable use of it. So I would expect economics to show it to be

true. It does not do any such thing—except for invoking introspection. I should myself think that introspection cannot register a lot more than something as broad and general as, say, the Smithian self-interest; certainly, so far as the demand side is concerned. At least my introspection does not hand over more than this to me; and in matters introspective one must assume more or less perfect democracy especially as I should feel hard put to it to consider the possibility of my introspection being too insensitive. Nor should I feel adequately persuaded to gulp down the suggestion that I had failed to appreciate the conditional, “If people were rational etc.” I have appreciated it all but the position is that one is not entitled to use the conditional—any conditional—for questions involving material truth without at some stage having shown what the conditional asserts as true, in fact, to be true. All this, at best, is pseudo-empirical. I realise that micro theory does not build castles exactly in the air, but I do aver that the (empirical) foundations are not what castles should feel confident about. The castle itself is nearly all deductive; and the castle is too well—ins and out—known to bear description. So I am on my third point. The castle, actually, is not what it is really supposed to be; it is, in effect, what, in the context, it ought to be. (I find the metaphor to be not very happy with the burden I am asking it to carry, but I still think it should do.) The castle, far from being an exact replica of its model, would appear to suggest that any discrepancies between itself and its model be made good by suitable changes in the model itself. No wonder, with little else but formal logic and value judgment to erect it, it does not need enviably strong foundations. (And I have done with the metaphor.) But I cannot dismiss micro theory’s value judgment as nonchalantly as I did its deductive exercise.

The basic ingredient of the value judgment in question, to put it crudely, is that since optimality is good the reality is optimal. This went along not too unsmoothly as long as the reality could broadly correspond to the sufficient condition: relatively free competition. Now, the sufficient condition is a combination of two necessary conditions: rationality and freedom. And if the system did in fact exhibit relatively free competition and inbibe a broad and general self-interest, the system *ipso facto* tended to satisfy the objective requirement of (which is not, necessarily, the objective of) optimality; tendency-wise (which is what would do), at any rate. But, while one can go on arguing about the fact or content of rationality, what happens to competition is more or less visible; which, together with certain internal logical inconsistencies of received theory, allowed for non-perfect competition situation to be accommodated in micro theory. But this

gain in empiricism had already been, at least, partly offset by the redefinition of rationality in terms, as required by marginalism, of maximization, a clear compromise on empiricism. The crucial point to be appreciated is that the relevant universe at the time maximization was postulated was a universe of perfect competition. This was the high point of value judgment; coincidentally (and significantly), the high point of utility as well. The optimal universe is no more. But the maximization postulate could not help continuing. I find it rather awkward to put it the way I am driven to putting it, but I cannot help: the value judgment has, as of today, little more than technical (method—wise) relevance; it is so much more, logically (mathematically), convenient to work with maximization. The Classical conceptualization of the tendency towards optimality defined in terms of the weak version of rationality (self interest) carried a much lighter burden than what the present-day micro theory finds its maximization postulate easily to permit. But this entails a difference which is more fundamental than what might appear to be the case: there cannot be a tendency towards something that is more likely to be false than true. Any presumption of the tendency would, implicitly, demand the value judgment. And the value judgment there is. Altogether, a not very positive economics, I should think.

But how is human behaviour the villain of the piece? It is not. It is what micro economics does with human behaviour that tends to rob the science of its genuine positivism: considering it, in affect, to be the variable to be explained rather than one of the explanatory variables. I should, indeed, have spoken of human behaviour to be the most important of the explanatory variables were it not for the fact that in any confluence of explanatory variables all are, almost definitionally, equally crucial. Of course, what is the appropriate confluence is a question of fact—what the relevant empirical inter-connections are. But assuming that technological behaviour is an element in the relevant inter-connections, who can say whether technological behaviour is more, or less, important than human behaviour? The immediate question, however, is, how is human behaviour not the variable to be explained in micro theory. After all, human behaviour is supposed, on excellent authority, to be the subject matter of the discipline itself. But this is precisely what I am questioning. Only, it is not necessary for my present purposes to go into the wider relevance of human behaviour. I should myself think, though, that macro theory, despite the excellent authority, is relatively free from the naked sway of human behaviour; the behaviour to be explained there is that concerning the national product. But if I put it this way, I must also

recognise that micro theory itself may be taken not to consider human behaviour to be the variable it purports to explain; the relevant behaviour there is that concerning the relative prices. Indeed, there is a close fit with macro theory inasmuch as micro theory is also supposed to explain, simultaneously with the behaviour of prices, the behaviour of the individual products—the composition of national product. If I were then, ignoring prices in an explicit sense for a while, to conceptualize the subject matter of economics, I should be tempted (and, I hope, not very unreasonably) to advance the candidature of product behaviour to be it; of which national product and individual products will be the two broad facets. Human behaviour, then, can only be accommodated as an explanatory variable. And I do not, in that case, notice much of the excellent authority.

Unfortunately, the authority has a deeper insight into the discipline's conceptualization of (micro) economics than the above appreciation would indicate. After all, what is there in quibbling over names. Relative prices, yes; composition of national product, yes. But these are mere consequences of human behaviour; deductive consequences, at that. Micro theory so well represents the conceptualization that the focus of the discipline's study is the relationship between ends and means, a focus which inevitably demands economizing and, as the next no less inevitable step, maximization. The inevitability, it is true, is logical; but, and this is the point, the logic looks swallowable precisely because human behaviour has been accorded the indulgence of the focal point. The only empirical inter-connection (and it is not true) that is allowed to keep awake (and how diligently awake) is maximization; all others are forced into deep sleep. What remains to displace human behaviour from the focus to the category of explanatory variables? The objection is not to the fact of the indulgence per se; any explanatory behaviour cannot but be fully absorbed by the relevant theory. The objection, right here, is not even to the suspect assumption that maximization is in fact the relevant human behaviour. The objection, right here, rather is that human behaviour, in terms of the relevant empirical inter-connections, is not the focus it has been made into; although the moment the discipline takes the crucial step of according human behaviour the indulgence of the focal point, the logical inevitability has received the invitation to show up. It is perhaps not fair to go into the motivations behind the invitation, but the unique determinateness which it so unobtrusively hands over is at once cheap and contrived. If planets exhibited alternate phases of acceleration and deceleration, the obvious ease of calculating constant acceleration cannot be the overriding consideration in postulating what

their behaviour in fact is. I have myself little doubt that economics has inhibited its explanatory potential by the complacency engendered by the logical elegance resulting from the focus on human behaviour, and that altogether new vistas of inquiry open up the moment the discipline decides to put product behaviour rather than human behaviour in the focus. The other relevant empirical inter-connections must be awakened—and something drastic done to maximization, too.

I must now introduce price—explicitly. I find that I must begin by distinguishing prices from the price mechanism. The point to be emphasized is that the price mechanism is, essentially, not a part of the problem of price determination. The price mechanism is market mechanism, competitive mechanism, competition; and the label price, there, is justified for the sole reason that it is through prices (via demand and supply, of course) that the competitive mechanism expresses itself. Irrespective of how they are determined, prices act as signals for the play of competition. Prices, as prices, are not the mechanism itself; exactly as the value of money, or how the value of money is determined, is not to be equated with what money does in the economy. The distinction is of some importance to the present discussion inasmuch as lack of unequivocal separation of these two, prices and price mechanism, is partly responsible for human behaviour being allowed to monopolize the focus. There is, as a reasonable hypothesis, a sound presumption that given relatively free competition and self-interest the system tends towards optimality, were optimality is to be viewed not as a rigidly defined objective but rather as a fact of experience. This fact of experience is, fundamentally, little different (and no disrespect to human beings is intended) from a similar fact of experience in the animal or plant world as a whole: the invisible hand is no less in operation in the world of grass-hoppers than in the world of human beings. But here the two worlds part, or can part, company. Men can turn the fact of experience into an objective, and, significantly, the objective is capable of being defined, defined in the most rigid as well as the most likeable manner. Now, even though prices themselves are not the competitive mechanism the more they are likeable (and rigidly so) the better they are as signals—likeable and better in terms of the already defined objective. The equilibrium price is eminently suited to the job, so because, backwardly (in terms of the propensity to optimize), it imbibes the ideal, the objective. All goes well while competition is as free as the definition requires. When, however, competition is not free, it does no longer allow the objective: one of the two legs of the objective has given way. But equilibrium itself is in tact. (The little



difficulty with oligopoly is different, and not quite disastrous.) Actually, this other leg is in tact only by assumption. Consequently, the equilibrium is as genuine or suspect as the assumption is genuine or suspect. I myself think it is suspect. Full circle of less than wholly positive, less than wholly empirical: a queer breed.

But, eventually, the prices and quantities that micro theory produces are the breed. In other words, they cannot be genuine. One way to make them genuine is to release them from the near total sway of human behaviour, from the maximization postulate. And this, unless the preceding discussion is a distortion, is unavoidable. Micro theory may, in the consequence, end up with determinateness with a range rather than a point; but the sacrifice in elegance is, I think, well worth making. I should myself think, however, that there will be a point, after all; only the point will be different from what the theory forces down its throat today. There is a point in the Classical natural price which is different from the one in the Classical market price; and the Classical price—market or natural—has a point different from that of the present-day micro theory's equilibrium price. Any equilibrium point is given by what is supposed to determine the behaviour in question. The relation may be causal as is the case with the labour embodied construct; it may be functional as well as causal as is the case with the Classical demand-supply construct tagged on to the relevant natural price; or, as is the case with the present-day micro theory's demand-supply construct, wholly functional. An equilibrium point is most likely to emerge; and the exceptions relate either to dynamical cob-webs or certain peculiar and perverse cases. But even if a point did not emerge, the phenomenon has to be accepted in good humour. And so with the stability of equilibrium. After all, neutrality and instability as well as stability are a fact of experience; and so of equilibrium. This, indeed, is the fundamental distinction between the layman's conceptualization of prices and that of the discipline's. The former looks at prices as they obviously are, the latter relates their obviousness to their potential equilibrium values. Probably, obvious actual prices are disequilibrium prices in the sense that they do not ever in fact coincide with their respective equilibria, but this only emphasizes the significance of what does not superficially meet the eye. The equilibrium is what rationality and freedom make it to be. If rationality is maximization and if it is perfectly freely expressed there will be one set of equilibrium prices; there will be different sets according as rationality is assumed to be less than maximization and freedom less than perfect. The same freedom will ensure a tendency for the actual prices to converge with their respective

equilibrium prices. And, in the last analysis, any one set is as notional as any other. It might appear that maximization and perfect freedom (competition) allow for a clearer conceptualization of equilibrium. Quite so; but one conceptualization is not superior to another. The question is one of specification of rationality and competition, the equilibrium is as easily conceptualized in one case as in any other. After all, the degree of competition element in the conceptualization has already been diversified; there is no reason why the same cannot be done about rationality. I realize the difficulty for mathematics, which already is proving impotent with oligopoly; but empiricism cannot be sacrificed for logical (mathematical) ease. I myself do not think that the theory's analytic construct necessarily needed to be displaced if it redefined the relevant function in terms of self-interest instead of maximization. After all, the Classics, too, had basically the same construct for their market price. Micro theory cannot (indeed, no economic theory can) assign specific values to behaviour relations; it can only say what behaviour relations imply what. Micro theory's trouble is that it wants to say more than it can. Why must it bind itself with maximization (especially as it is worse than suspect) when all that it should feel called upon to ever is that rationality has a certain explanatory implication, ranging from, say, non-negative self-interest to maximization. Micro theory bravely faced up with its analytic as well as empirical inconsistencies as regards competition; time it did so as regards rationality as well.

I should myself expect the other relevant empirical inter-connections automatically to wake up as soon as maximization, and through it, human behaviour, is put in its proper place. One has only to recall what happened as soon as micro theory explicitly allowed for non-perfect competition to find a place in its theoretical system. After all, all the various empirical inter-connections that were then assimilated by the theory (the theory of the firm area of micro theory) did not spring up overnight; they were all there already as, indeed, they must be before a theory (true theory) can incorporate them in its theoretic structure. It is this possibility that I had in mind when I spoke of the altogether new vistas of inquiry opening up before micro theory.

I could promise more. And I can take the cue from the theory of the multi-product firm: has it not only turned more realistic (and it has yet to go on in the same direction a bit more), it has also come closer to a micro-macro inter-relation. I expect a similar macro-ward approach as soon as micro theory leaves its maximization straight-jacket behind. This is important in itself if only because I should

like micro theory to join in the over all theoretical system of economics in a more integrated, organic, manner than it would appear at the moment to allow itself to be. I concede that macro theory, too, has to do something similar about it, but one cannot afford to wait for the other to make the necessary move. There is, moreover, a greater compulsion for micro theory to make up its mind. It either does what I am suggesting, or it must remain little more than an analytic apparatus, which is what it is, rather than aspire to be a theory. Even as an analytic apparatus, the present-day micro economics is overburdened with maximization; consequently, it is a less useful apparatus than it needs to be. It is, at the moment, not even strictly speaking, micro. It is as much a "theory" of relative prices as of the price level and indeed the whole conceptualization of the competitive mechanism, with or without (or with more, or less, of) value judgment. The Classical economists would have been flabbergasted if told of a 'theory' of the competitive mechanism, or, of its being co-terminus with any theory of value. Their conceptualization of the mechanism, essentially, stands apart, despite the fact of the prices acting as signals through which the mechanism expresses itself. As a matter of fact, even their theory of value acted as the central analytic instrument for their main, macro, preoccupation. The mechanism, in other words, is a postulate; the theory of value, an analytic apparatus. Only, they did not demand from their theory of value more than it could supply; it is no mere coincidence that they did not expect their theory to hand over the quantities of the individual products, the composition of national product, with the definitional automaticity the present-day micro theory does. And, then, they have some such thing as the natural price, which is outside the system of market prices—whether in the entwined, all pervasive, mechanism (of which prices were but part) of Smith or in the explicitly outside element, labour-embodied. Nor is it a mere coincidence that the Classical involvement with the measure (the invariable measure) problem was, while conceptually independent, quite related to their involvement with the value problem proper. It may well be that micro theory is best conceptualized as a "measure envelop", *i.e.*, a box of analytic tools, (an area which develops the measures and techniques to be used by the discipline as a whole) which prepares the grounds for the real preoccupation of the discipline, the behaviour of national product. But my main purpose here has been to plead for injecting into micro economics as much of positivity as is possible under the circumstances. One can wait a while to be able

fully to decide what the exact potentialities of a positive micro economics are—as (an empirical) theory and/or as a box of analytic tools.

## CHAPTER 9

### Reflections on Economists

Who—what—are economists? The economics Nobel laureates? Smith, Ricardo, Walras.....? Anyone engaged in economic theory-making? Anyone with command enough over economic theory to be able to engage in economic analysis? Aristotle? Veblen? Russell? Marx? Professors of economic? Economics tripos holders? Mr. X who died very very old with one publication to his credit, a 79-page piece on the size distribution of potato farms in the village of Potati? Mr. Y who died very very young with one paper to his credit, an unpublished 9-page piece on the hypothetico-deductive elements in the alternate constructs of comparative disequilibrium dynamics? Of course, there are economists and economists, and, of course, there is no alternative to defining the intellectual personality of the subject(s) according to one's own predisposition. This is what, in the following pages, I propose to do.

To me, an economist is one who engenders simplicity in the theoretical system of economics. The terms of my(?) proposition are nearly well defined. But I should prefer to restate—to project exactly what I am after. First, a minor point almost of dictionary-wise meaning of 'to engender,' though. The meaning that I want to attach to the verb is, if I may say so, one of qualitative addition towards (I have yet to restate) simplicity. In other words, I presume that there already has been a theoretical system of the discipline and that it already to a certain extent imbibed simplicity. My presumption, if a fact, no more than expresses the basic property of science; science implies, is, theoretical system. And system implies simplicity; a certain minimum simplicity, that is. I should myself have very much liked to trace back the emergence, the very first coming up, of the discipline as (even as) a rudimentary system with little more than notional simplicity. But I find my scholarship to be a little too constricted, and my propensity to widen or deepen it rather weak and low. And, in any case, there is still so much to engage my predisposition. I shall, however, allow the verb a full go for the future:

the total supplanting of the existing system by (in the nature of the thing, obviously) a much (I am sorry to have anticipated) simpler system.

And now to "theoretical system", and "simplicity." A system denotes an organic unity among the parts of something; an integrated whole, an inter-relationship, a chaining. The human body represents a system; so does a chain, an institution, a construction. The functional as well as, if relevant, the physical relationship is subsumed in a system, the emphasis depending on the context, the universe of discourse. A theoretical system, then, is a body of theories such that the theories are more or less inter-related, inter-linked, among themselves. A science, which is what a theoretical system refers to, may be conceived of, to use a picturesque expression, as a chain of theories. But, the rings of a chain, the various individual theories constituting a system are, of course, by no means all equally intimately inter-related to one another. A theoretical system may thus be understood as made up of groups of theories such that the theories in a particular group are more closely interlinked with one another than with those in another group; and, understandably, the theories even within a particular group are not equally closely inter-related. The groups themselves can be thought of as envelops (envelop-chains?) or sub-chains such as, for instance, theories in micro, and in macro, economics.

Simplicity? Clearly, this is an attribute of a theoretical system, and is perhaps more easily understood in terms of relativities. But if I were to express it in absolute terms, I would describe a simple (which here, in effect, connotes "the simplest possible") theoretical system to be one where the formulation of the theoretical structure can be said to be truly circular, in the sense that the explanatory (theoretical) system exhausts as well as it is exhausted by the range of behaviour called for explanation. Naturally, this can happen only if any distinction between the relevant explanatory variables and the relevant variables to be explained (as imbibed by the theories constituting the theoretical system, science, in question) is little more than expository, since the elements (variables) in the two categories, in a reversible functional relationship, as it were, get explained by one another all at once. More generally, one theoretical system is more simple (simpler) than another if the various different theories, and groups of theories, in the former imbibe, and exhibit, the property of circularity more markedly than those in the latter. It is assumed, of course, that the circularity, simplicity, is genuine, not apparent or contrived, and, in an empirical science like economics,

not definitional or (in a sense, worse) pseude-empirical. It is probably the case that the physical sciences are simpler theoretical systems than the social or, even, the biological sciences.

But, of course, a theoretical system is as simple, and the simplicity as genuine, as its constituent theories are genuine and simple. A word each about these, and I shall confine myself to empirical sciences. Concerned as it solely is with material truth, an empirical science admits to its system only theories which carry material truth relevant to its term of reference, the behaviour to be explained, the behaviour the theory is about. This suffices for genuineness. There may, however, be more theories than one with the same (originally, at any rate) term of reference. Given that they are all genuine, one rather than another may still be preferred if the former is simpler than the other, in the sense that it integrates with the existing system more easily, or closely, than the latter. But there is a catch. It is not impossible that a theory which does not do either (on the contrary) may still be considered simpler if it turns out to be capable of explaining a wider range of behaviour than what prompted its hypothesizing in the first place, and more (materially) truly than the already existing theories in the system; if, in other words, it displaces some already "systematized" theories as well as its immediate rival. It must be a very powerful theory, indeed. And anyone who put forward such a theory in economics must be an economist; even to me.

Such theories, and such economist, are always, even in the best of circumstances, few and far between. It should be instructive, even impressionistically (which, indeed, is what I can here manage to attempt), to look at some of those who can be said to fall in the category of simplifiers—to see what is it that they have done, how they have done it, and, possibly, when (it is mouthful) is it most likely to be done. Given the standard of reference, I find that I must begin with someone who it considered to be, to confine myself only to three ways of description, more than an economist, no mere economist, and less than an economist. It is of course Marx. But I also find that my restatement of the terms of my proposition cannot contain his case. Fortunately, it implies what can; so let me elaborate—and tersely. If individual sciences are conceived of as distinct theoretical systems, Science (with a capital S) is itself, without considerable strain, conceivable as a grand theoretical (explanatory) system. And assuming that there is nothing illegitimate about it, the concept of simplicity is easily comprehensible when applied to such a grand envelope (of which the individual sciences may be treated as sub-envelopes). After all, human knowledge, the genus of which the

sciences are a species, is, fundamentally, one and indivisible; and in any case, the social sciences cannot but have a certain theoretic affinity among themselves. This should do.

I have, in effect, implied that Marx was a simplifier—and, indeed, a grand simplifier. In his grand version, Marx put forward a hypothesis (as well as a scientist of the social universe can) on the evolution of society in terms, basically, of economic relations which allowed him at once to explain the mechanism, inner logic, of the functioning of the capitalist economy and to appreciate the processes through which society had journeyed on to the capitalist phase, with implications for the shape of things to come. In this, he was actually a system builder rather than simplifier; so because his hypothesis did not displace any other hypothesis, or system of hypotheses, if only because none in the relevant category really existed before. He constructed a theoretical system almost *ab initio*, of the science of society (which, of course, is neither social science nor sociology). But he was, in a sense, a potent simplifier in his non-grand version. He had to be. It was a logical as well as an empirical necessity for him. For, understandably, he could not possibly countenance such of the economic theories (those that he could not in honesty ignore) that were logical, and, if I may say so, empirical, contradictories of the fundamental propositions that he required for his conceptualization (hypothesis) in his grand version. If class struggle were to remain the prime mover of the social process the implicit tranquility of the invisible hand and the smoothness of the Say's law could not but be anathemas; nor Smith's productive-unproductive labour distinction but incongenial (after all, liverymen as well as hateful grand dukes had been put together). Ricardo was another matter, the embryonic conflict-elements all easy of development, and the latent analytic weapons not too difficult of surfacing and sharpening. The very conceptualization of the discipline, in terms of distribution should have been enough to endear Ricardo as a so very kindred spirit. And there was so much more there when one came down to details, not excluding the Chapter on machinery. But, then, I cannot improve upon the easily available vast literature bringing out the inter-connections between Marx's needs and Ricardo's riches. But Marx could tell Ricardo off if he needed that too. For instance, the latter's shilly-shallying about the theory of value, to-ing and fro-ing between labour embodied and plus profits, was too frail and mercurial a conceptualization to carry the weight of surplus value. Marx never intended to simplify the theoretical system of economics; scarcely a single of Marx's theories was meant for its own sake, or for displacing an existing theory for



the sake of displacing it. Certainly, Marx's universe of discourse was different from the economist's, which is conceding enough—to Marx. Also, his particular involvement cannot be made to go against him, and those of his theories that, in spite of his involvement, belonged to economics proper must be assessed in their own right. Such, for instance, is his formulation on business fluctuations, integrated with his wider theoretical system, but very insightful in itself. And except, essentially, for Malthus, Marx was an originator there, too. The question of his formulation displacing something else on the subject is, therefore, not particularly meaningful. Business cycles at the time were hardly an important, consciously felt, problem for the society at large, and not at all a problem for the discipline. Marx, the potent simplifier as an economist, was abortive; nearly totally in his own and until recent times. Even his resurrection, by way of a growing appendage to, rather than assimilation—displacement within, the existing theoretical system of economics, tends to engender almost the opposite of simplicity. One almost wished that the “more than, less than, mere” were motivated slanders. But Marx the grand system-builder remains unsurpassed.

The lessons? I cannot help going beyond what the foregoing in itself entitles. If you do not own an already existing theoretical system, the system will not own you; and if you are building a system of your own, it all depends—on whether the system gets recognised and further developed as a distinct science. Of course, this, as already so well established in Marx's own case, has nothing to do with your recognition outside science—in the wider world of social existence. But to be able to build a grand system, you have to have grand vision. Involvement with facts alone may make some sort of a scientist of you; it is possession of a grand vision, given that you know your facts as well, that can deliver a truly great scientist. Marx had this in enviably great measure. It is, of course, an advantage also to be able to evolve new analytic techniques. Marx's reproduction schema, which represented an empirical reality as well as a method, may or may not have been a total innovation, but it was a considerable innovation. Above all, perhaps, grand vision can emanate only from deep involvement, not only in your own universe of discourse but also, in some aspect of the social universe, the universe you live and work in as an individual human being, something that, without making you bitter, hurts you, haunts you; and the more it hurts and haunts, everything remaining equal, the greater the chances of the vision emerging. Marx, on all evidence, had this, too. Marx, curiously, was, on my reckoning, not a simplifier. But he had

all the attributes that go to make one.

I must conclude this appraisal of Marx by making a confession. I am aghast at my own conclusion. The acuteness of my mortification will be better appreciated if I confided that, in choosing him to be number one of my subjects, I had (subjectively, of course) already concluded that Marx was the greatest simplifier of all. I recall having elaborated my restatement of the terms of my proposition to be able to contain Marx's case. As it is, economics cannot contain Marx—and Marx could not have been a simplifier as an economist. Shall I attempt a fresh elaboration? I would rather not; quite honestly, I do not think I need to. I take my conclusion sportingly.

What of Walras? Walras, beyond question, was a simplifier. The simplicity engendered by him was based on utility, as against the classical (in one form or another) cost-based value theoretic structure, as the most relevant prime mover in the process of exchange and, therefore, in the universe of economics. But the novelty was not so much in the empirical element introduced (utility), as in the method, marginalism, that allowed the element to be handled with felicity, especially, in the conceptualization, and unique determination, of equilibria. The equilibrating vehicle is the demand-supply construct, one that the Classics used—or for that matter any economics must use. Already, the story, with all its details of plot and method, is part of the economics folklore, and cannot bear repetition. His status as a simplifier would probably have been greater than, on this count, it is, if Menger and Jevons had also, and about the same time, not been his co-simplifiers in precisely the same respect; what they together achieved, that which came to be christened neo-classical economics. Altogether curious; but, I suppose, we are all human—those who judge as well as those who are judged.

But why neo-classical economics? This, too, is folklore. But the question might still bear fresh attention. The simplicity engendered was in the area the Classics understood as the theory of value. That the new theory should displace this part of Classical economics is understandable; how effectively, and in what particular respects may be treated as a matter of detail. But this in itself does not, cannot, explain the impression that a new economics had displaced the old, impliedly in its entirety. The new supremacy of utility over cost could not in itself be the decisive element, although utility, in the new appreciation of it, had, as already indicated, acquired near-omnipotence. How possibly could the new construct, essentially a micro construct, have, however, either of logical or empirical necessity, even notionally displaced the Classical, essentially macro, conceptualization

of the discipline? The objective economic situation obtaining at the time entailing a greater emphasis on allocation rather than on growth (which, admittedly was, with industrial revolutions, colonies and all that, going on well enough; people, economists included, not being too involingly conscious of the occasional jerks in the process), was relevant, but not the definitive element either. Nor, indeed, were any political, capitalistic, motivations allegedly corrupting economists of the time. One element of the explanation was the excitement that marginalism (marginalism, *not* utility) generated, the fresh avenues of analytic exercises it opened up, and the time and space it all consumed, leaving comparatively little scope for the old and familiar stuff. Classical economics, or simply economics, moreover, had in one respect (especially, the uninformed as well as informed criticism of the Ricardian position) been tattered, while in another supposed to have reached (with J.S. Mill) its culminating point of excellence. The latter (the supposition) was never widespread, and it is not unlikely that none else but Mill was convinced about it. But it is a fact that, even before Walras and his company generated the excitement, interest in the main preoccupation of the Classics, macro economics, had reached the vanishing point; in terms both of method and empirical formulations. Few, not excluding the Classics themselves, had grasped the analytic potentialities of Quesnay's *tableau economique*. Marx, the economist, had remained mostly unpublished or untranslated; but he might not have mattered any way. Most controversies had centred around the theory of value, and macro economics had remained virtually an ignored baby. The macro economics of the Ricardian conceptualization (to be taken up later in these pages) was being developed with his own slant by Marx, but we have already seen that Mill's supposition had an impressionistic truth about it, after all. What, however, is crucial for the point I am presently developing is, not the why but, simply the fact of the impression that macro economics, tattered or culminated, no longer registered the impression of a striking existence. So when the innovation came, in displacing the micro part, so to speak, formally, it *ipso facto* displaced the whole of Classical economics. A rather curious and intriguing simplicity.

The simplicity, at all events, was fundamentally, analytic rather than theoretical. But to say this is, essentially, to reflect on the nature of the theory of value; it is true not only of Walras' theory but of the theory of value per se. The question is whether a theory of value can escape being definitional, or almost so. The Classical theory has had the advantage of the dichotomy between natural price and market

price where the demand-supply construct was explicitly relevant only to the latter. The neo-classical theory dissolved the dichotomy, unless one argued that the Classical natural price conceptualized in terms of cost was replaced by an implicit natural price in terms of utility. In any case, the demand-supply construct became the sole instrument of handling the price problem. And the demand-supply construct, despite its relation with real (empirical) forces, cannot in itself but be an analytic, as distinguished from an empirical hypothesis-based (theoretical), construct. And Walras (in the empirical sense of the term, the only admissible sense for an empirical science) not a theorist? An anti-climax?

Lastly, I have to say a word about the rather eager, expectant, embrace accorded to utility. I should like to suggest that fascination for a single-factor explanatory conceptualization is, in a manner of speaking, in-born in man, supremely exemplified by the God model which man postulated to explain everything in time and space. It is always easy of comprehension, analytic handling, and all in all a contributor to elegance. I have in mind the single-factor value determinant. While labour-embodied as the sole value determinant began, notionally at any rate, with Smith, it is there shown and shut, and an almost clear invitation to ignore it. In Ricardo, the stuff is (Smith might have felt embarrassed) singled out and hammered. But Ricardo dilly dallied and, the counter attack made easier, the conceptualization got diffused, and the impact on the fraternity weakened. Marx returned the blast in full swing; too naked perhaps to suit the refined sensibilities of the tribe, however. But utility was an equal match. This did it—for concentrated impact; with all the advantages of the exploits of the marginal tool. Lesson: you have to have a restrained, refined, sophisticated, rather cool, expository disposition to be appreciated. The style may now and then be sharp but not too, or obviously, sharp. And no jumps in deductions will be tolerated; the implications must be slowly, visibly, demonstrated. Lesson: you will do well to develop, and use, mathematical expertise.

But Walras did something else as well—and did it alone: the so-called general equilibrium system. It was an analytic system, although, of course, there was (there had to be) a theory behind it—more or less the theory the neo-classical economics represented. His neo-classicism, relegation of macro economics to a virtual non-entity, is fully reflected in his general equilibrium system as well: it cannot possibly illuminate, even grasp, the picture as relates to the aggregate of quantities, the national product. It would appear that the system cuts across the distinction between micro and macro entities, in that

no prices and no quantities are left out: the whole of the economic universe would seem to be exhausted. The distinction between the two types of entities is, in effect, dissolved. However, while the assimilation of all conceivable elements is complete, the assimilation is no more than, what may be called, horizontal; there is no vertical assimilation. Or, to put it differently, the assimilation is quantitative, not qualitative. So far as the determination of prices and quantities is concerned, the system is in every respect complete. But it is perhaps beyond the internal logic of any simultaneous equation system to imbibe the properties of an explicit explanatory framework for the behaviour of the totality (national product) as well as for the behaviour of the parts (individual quantities). Nor perhaps can the system handle the macro relations insofar as they relate to the direct confrontation between national product and money; the price level is in essence a sleeping entity. The determination of national product itself would demand explicit handling of such variables as aggregate demand and supply, savings, investment and so on, which the system cannot possibly do. His, in this respect, is a contribution in method. A system-builder, unlike Marx, within economics, Walras' system, unlike Marx's, was analytic rather than theoretical. The impression of an anti-climax persists.

And Ricardo? A real simplifier, in conceptualization of, as well as in respect of most of the major individual theories in, the theoretical system of economics as it stood at the time. Of course, his conceptualization of economics as a science, in terms of distribution rather than production, is easy of misunderstanding and misplaced emphasis. Actually, his conceptualization was hardly less involved in the behaviour of national product than any other before or since. The emphasis simply was on what he considered to be a profoundly significant explanatory element in the understanding of that behaviour: how what happens to distributive shares makes all the difference to what happens to national product. So he was naturally interested in why the distributive shares behaved the way they seemed to do. It was in this, and only in this, sense that his conceptualization, which he so loudly pronounced, was in terms of distribution. Any misunderstanding on the point, I should think, was, the paradox notwithstanding, largely due to the master's explicit and categorical differentiation of his own conceptualization from that of Smith's (after all, the great old man, Smith, had virtually passed by it—had hardly perceived the very potent implications of the inter-connections). His differentiation, unfortunately, was more categorical than explicit. Take out his assertion about his position, and, I venture to suggest, one is not at

all likely from reading him to get the impression his assertion imparts. Lesson: It is not always safe to go entirely by a master's remarks in isolation from his detailed exposition that may or may not, on one's own reckoning, substantiate the remarks.

But this general appreciation of Ricardo's conceptualization of the discipline has implications for the theoretical system of economics which must now be uncovered. The conceptualization entailed a re-structuring of the system that involved more than reorganization. How the various different theories are to be chained (inter-linked) in groups (envelopes) and how, in turn, the envelopes themselves are to be chained. Yes. But not just this. Also what gaps in chainging the re-structuring may have shown up or, even, occasioned. After all, the need to re-structure is a consequence of the conceptualization, and which, therefore, was the standard of reference for the restructuring. "To be chained", indeed, is not the true description; what a conceptualization tells is how the theories "are chained." It was this that Ricardo might have made explicit. But, as in several other respects, he probably considered such explication pompous, or perhaps not in intellectual taste. He would certainly have felt called upon to make it all explicit, had James Mill sought clarifications or Malthus engaged in disputation—not on the general appreciation, but, rather on the implications, of the conceptualization. I wonder if it ever occurred to either to read him as his conceptualization demanded: not theories, but the way he saw theories being inter-linked; the system. Whether and how his *Principles*, in fact, exhibited the consequences of Ricardo's conceptualization is a slightly different matter; he may or may not have succeeded. But any judgment, and, indeed, any sympathetic (in the objective sense of truly trying to understand) reading of Ricardo, demands an awareness of his conceptualization, and of its system-wise implications. I would myself expect Ricardo to have expected of himself to project his conceptualization in his *Principles* as well as to have expected his readers to expect of him to do so. Ricardo, I am sure, meant what, as I put it, he so loudly proclaimed. Make his assertion about his position as explicit as I have indicated, and, I venture to suggest one is very likely to get the impression his assertion should impart. Lesson: it is not safe to be skeptical about a master's remarks on matters fundamental to his approach, which, indeed, should be taken to be providing an eye for reading him. Footnote: there is no conflict between this lesson and the proceeding one.

Background enough for beginning to read Ricardo from this stand-(vantage?) point; with this eye. I cannot myself here begin to. But I can promise anyone prepared to exploit the standpoint that he

is more likely than not to find that it was in what Ricardo accomplished in terms of the inter-linkage among theories, and among groups of theories, that he comes out to be, upto-date, the greatest simplifier of all. I can be a little more helpful. Ricardo's initial uncertainty, even reluctance, about whether to engage with the price problem, the considerations which really induced him to be, in this respect, more willing, the way (ways?) in which he faced upto the problem, the reasons for his possible, and apparent, shilly-shallying between pure labour embodies and plus profits (rent, in any case, being out of the picture); his excitement about the new-found West-Malthusian theory of rent; the incessant tug of war between wages and profits (Ricardian vice?); his stationary state; his reappraisal on the question of machinery; and his life-long struggle with the question of the invariable measure, problems that engaged Ricardo's attention in and outside his *Principles*, and why he preferred one solution to another as also why he solved a problem the way he did, can be seen, if one held on to his conceptualization, in a delightfully (if also provokingly) different light from the one (ones) one is very likely to be accustomed to. Smith's amplifier, not one who can work with his own clay, Jewish, stock exchange, rich, his circle (School and followers)...stuff and nonsense; you will discover for yourself. It may also be noted that his struggle with the measure problem did not stop him from doing what he was otherwise doing. So perhaps measures and techniques are a help but you can do with less than what you might have had. I should also like to emphasize that, naive views about his being deductive notwithstanding, it is hard to come by a more robust empirical economic theorist.

Of course, even if my promises turned out to be true, Ricardo's theories would have to be judged, not (or not only) for having reflected his conceptualization, but, for whether they are themselves true, materially—at any rate, how (in a broad sense) their probability value compared with that of the other theories on the relevant problems. He could not possibly be infallible. Indeed, he (his theories) can be said to be prone to have gone wrong on two counts, for the ever-present possibility of a theory being false and, additionally, for his theories to have had to integrate with his own conceptualization—where others (their theories) are not likely to suffer on the latter count.

It is interesting even suggestively (which is what I can here afford anyway) to compare Ricardo, Marx and Walras. Marx, for the reasons already indicated, was, for the theoretical system of economics, the odd man out. Walras (ignoring the here ignorable General Equilibrium system), together with the other two gentlemen of neo-classi-

cism, tinkered with what at best is preliminary to the economist's main preoccupation. Indeed, if Marx was abortive as a simplifier, Walras was (I realise how blasphemous would it have sounded to Schumpeter—may God rest his soul in peace) not *even* that: looking back, there was little promise with his chosen involvement. Ricardo, unlike Marx, was so much 'in'; and unlike Walras, was involved with the broadest *possible* involvement of economics as a science—"in", unlike Walras, in the only way a simplifier (certainly a great simplifier) could be in. If Marx was not really "in", Walras was not sufficiently so. And Ricardo? Well, we have already seen that. He was a great simplifier, the greatest—but I have already said all that. And to me (as per my proposition), an economist.

Was Keynes an economist? Well, it depends. Was he a simplifier? He was. Well, he should be. Let's see. I can be short—for the reason that we have already assessed as many as three others; it (the possibility of apt comparisons) is a great advantage for natural brevity. I could, in the bargain, talk of things that I would otherwise not have. Keynes had the minimum necessary propensity, to the extent necessary for him, to grasp Marx as well as those specifically of the economic tribe. He also was conscious of, and, I am sure, had (in his own mind) formulated about, fate—of what makes for success or otherwise; and he would seem to have meant to be a great success. He easily avoided being Marx; in temperament, experience, even scholarship, if not also perhaps in his particular nationality, he could not possibly (as I think he must have reckoned) be "overdoing" (in science or sentiment) as Marx could. I am not even sure that his *General Theory* was occasioned by his concern for the society as well as for his science (I would not add, or for himself, since we are all human). And despite being 'out' in more ways than one, Keynes was always, or nearly always, "in." But if he had a rather negative predisposition towards grandiose, it held for as (compared with Marx) smallish a grandiose as the stuff like Walras' General Equilibrium System. Keynes, the mathematician as well as (here used in the ordinary sense) economist, might well also have seen that Walras' system was, essentially, a method rather than theory. The same could also be true of his attitude towards Ricardo's wide economics orientation. But perhaps he had not tried himself to conceptualize the discipline as a whole any deeply (an apparatus of mind etc. notwithstanding). Or, also perhaps it looked odd to him to produce a general *Principles* (too unsophisticated for him). But he was sufficiently interested in some sort of 'generalness' to have found it possible, by the 'General' of his *General Theory* to make for what he was dispositionally, or



expertly, incapable of. (It is stale story now about his being conscious—his letter to Russell—of what great a thing he was about to give to the world. He might then have considerably reflected over the title of his projected book. Relativity was out, would in any case have been inappropriate. Walras' system, for title, should have been attractive, and appropriate. Or perhaps nothing of this all).

From my own (my proposition's) angle, Keynes was, in terms of the manner of his achievement, closer to Walras than to Ricardo, in that he opted only for a part of the theoretical system of economics. In terms of achievements themselves, however, he was closer to Ricardo than to Walras, in that he should have had thought about the discipline well enough to be able to apprehend the merit of opting for its main preoccupations rather than the preliminaries. The preliminaries, moreover, are in any case less than totally empirical theory, and a total involvement in it by anyone may prove hazardous (lesson ?)—the real issues are sure to be left out. He had, moreover, already had an almost life long taste for, and work on, things quite neighbourly. And the objective economic situation was crying for a way out. Between Marx and Malthus he was sufficiently well provided with the rudiments of an alternative formulation on economic depression, Khan's multiplier came handy, Say's law the analytic starting point of attack, and his logical equipment was, of course, there. He came out with a theory that was simpler than any existing, and it easily displaced all and sundry; it was also sufficiently well integratable with the theoretical system of economics. The theory was powerful enough, indeed, to engender simplicity in the system, in the part which represents a sub-group (sub-envelop) of macro statics. One could easily perceive what had emerged, how had it displaced what, and how it got integrated with the overall system. Keynes was a considerable simplifier. He was an economist.

Time I got hold of the great old man, Smith. I might already have been giving the creeps: was I, would I possibly be, ignoring the father of etc. Wasn't he a simplifier, after all—not an economist? Let us see.

Compare him with Marx. They are both system-builders. Smith might well have, if he had, and he very well may have, made economic relationships the main (or even part) determinants of, based on his *Theory of Moral Sentiments*, and would-be science of morality rather than, which is what in fact he did, making the former, in a sense, part of his theoretical system of economics, mat Marx's fate, too. As it turned out, he owned the science, and the science owned him. If he did not exactly father it, he certainly helped it grow in the

earliest of its childhood: any dangers of infant mortality were out. He also gave it an orientation; first of eclecticism and breadth of vision and earthiness, and then of integrity, its parts to be sufficiently inter-linked to form into a system. It was in doing the latter that he sifted and sorted from out of a variety of alternatives, and what he achieved was engendering simplicity to the system as a whole, and to the groups and sub-groups, in a good measure. But his eclecticism did not stop him from being rigid about his views on mercantilism, nor his breadth of vision persuaded him to take kindly to the whole lot of his unproductive labour, nor indeed did his earthiness stop him from soaring high into the skies when it came to the conceptualization of equilibrium or the elegant inter-connectedness of the elements of the economic universe. Everything has limits, after all. But, of course, the decisive element, in what to have and how, was his conceptualization of the discipline. The conceptualization, basically, had two components. the mechanism, or perhaps the spirit of the mechanism, was the invisible hand, and the focus, national product. Everything could now be put in the right boxes. Division of labour represented the technical, and profits the behavioural, accompaniments of the process of growth of national product; population growth, wages, and technology provided the meeting points of the analytical relationships, with hypotheses on each and, especially, on their relationships.

He did it well, especially, considering when he did it. How did his accomplishments compare with Walras'? Not a stickler for method, techniques, nor narrow in conceptualization of the discipline, he had his own general equilibrium system and basically the same as that of Walras' except for being anything but mathematical in exposition and, more significantly (so significantly, indeed, as to make the comparison seem contrived), while the macro elements together with pricing and an implicit allocative mechanism were given full play (they were indeed very much the bases), the focus was on the behaviour of the economy as a whole, which was (is) different from the sum of the behaviour of the parts: aggregation and aggregative behaviour is not the same thing as addition and a statistical average of the behaviour of the parts. With Ricardo? He would have felt amused for more reasons than one (why, for instance bother too much about the invariable measure, mine was good enough, operational or otherwise notwithstanding—it's basically something notional, after all; or, you have misunderstood my point about the non-labour embodied stuff—it can't be dismissed on merit, empiricism or logic) and happy for no less (you have made the child grow, I dare say).

What had been the contribution of mathematics in the making of the five? Of these, only Walras and Keynes were mathematicians as well as economists. Marx is an altogether different category, and may well be ignored. Keynes had greater mathematical expertise and certainly much deeper as well as wider association with economics. But he did not make much use of his mathematics, and made little show of it, in his economics writings. He must have had his own reasons, but there was one that was there for him to see: Marshall the mathematician is not too visible in Marshall the economist. But Keynes could not possibly have not made use of his mathematics as an economist—in reflection, in formulation, and in examining the implicative relations. He probably did act on Marshall's example; in fact went beyond: as regards mathematical appendices or in-between mathematical, even geometrical, formulations or reformulations. Walras, of course, was, in this respect, the exact opposite of Keynes. We have already compared their achievements. What can be added? What can be added will not be specific to them alone: mathematics is an advantage for any scientist, but it cannot be a substitute for empirical insights, although even in the latter mathematics could be quite helpful. Mathematics cannot provide conceptualization, nor choice of the area of involvement, nor problems, nor hypotheses for problems, although in these, too, mathematics can be helpful. But it is only after all these are there that mathematics can come in on its own as a decisive instrument—as the reliable judge of implicative relations. There is, indeed, the danger that an undue consciousness, and use, of this power may well tend to make one's interest as well as actual involvement confind to, or at any rate concentrated on, matters basically logical rather than empirical. Walras may or may not, in fact, have suffered on this count, but this is a possibility that may well be kept in mind in assessing him; and not only him, but all of his like. I have already implied why Walras' case need not be applicable to all economists with strong mathematical expertise. Keynes proves, not the rule but, simply, the fact of the implication: the creative attributes (conceptualization and the like) are of the economist proper, who needs mathematics (logic) as he needs ordinary language—in a special sort of way and with a special purpose perhaps, but, fundamentally, similarly. Smith's and Ricardo's handicaps are obvious; but also obvious is the strength of their creative attributes. Lesson: you will do well to prefer a little more of the creative attributes to, relatively, a little more of mathematical expertise. Footnote: one can be cipher in a subject but, unless one were an idiot, one cannot help having, even without knowing it, some mathematical (that is, logical) expertise.

I had better call it a day. Of course, there are many others among the tribe, living as well as those not living, who could be considered as simplifiers—not many who displaced conceptualization or a whole envelop-chain, but surely those who displaced individual theories or put them forward for the first (or almost the first) time. But even if it were otherwise possible for me to attempt such an almost impossibly large assignment, my scholarship, I know it for certain, will fail me. I may already have been too demanding with it.

## CHAPTER 10

### The Economist as an Intellectual Dilettante

Becaria—a penologist of international fame at 30, economics chair at 50, also in Milanese administration, reformer, wide range of intellectual interest, writer on varied subjects including aesthetics, mathematical equipment; Bernoulli—eminent scientist, work on theory of probability; Bohm-Bawerk—legal studies, public servant, cabinet office, economics chair, wide intellectual interests; Cantillon—banker, intellectual peer, economics writing only four years before death, mathematical equipment; Cournot—administrator, Professor of analysis and mechanics, rector, work on probability, philosophy, epistemology, economics publication at 37; Edgeworth—mathematical equipment; Jevons—civil servant, teacher, logician; Keynes—mathematician, work on probability, civil service, wide intellectual interests including the fine arts, business instinct, powerful conversationalist (Bestrand Russell recalled carrying his heart on his palm whenever engaging in argument with Keynes), Cambridge don, prolific writer; Malthus—clergyman, professor of history and political economy, author of the famous essay on population at 32, economic argumentation with Ricardo; Marshall—mathematician; Menger—briefly civil servant, economics chair at 33, careful thinker, not strong mathematical equipment; Mill—one of the chief intellectual figures of the 19th century, involvement in current issues as well fundamental questions, great works on logic, philosophy, economics; Pantaleoni—man of many activities and wide intellectual interests, both scientific and non-scientific; Pareto—engineer, mathematician, man of strong passions, solid classical education, economics chair at 45, retired early to work in solitude; Petty—physician, surgeon, mathematician, theoretical engineer, member of Parliament, businessman, (according to Marx) founder of economics; Quesnay—surgeon, physician, distinguished professional career, first physician to the king, medical treatise on bleeding, eminent in high intellectual circles; Rae—classical scholar, intellectual refinement, mathematical equipment, biologist, physician, widely traveled, wide intellectual interests and

writings, no more than a saving knowledge of economics but deep penetration, novel ideas; Ricardo—business career at 14, broker, jobber, operator on the stock exchange, made money and retired at 42, little scholastic education, no wide sociological conceptualization; Say—teacher of economics, businessman; Smith—very wide knowledge, work on diverse subjects including moral science, languages, astronomy, called father of economics; Thunen—education at agricultural college, farming at his own estate, intellectual work, strong propensity towards generalizations, first to use calculus as a form of economic reasoning, original in method and thinking, had his formula on natural wage engraved upon his tombstone; Turgot—educated for the Church, abbe, master of wide intellectual horizons, Minister of Navy, Finance etc., great civil servant, wrote on ‘existence’, ‘expansibility’, etymology, philosophy; economics work at 42, (with Beccaria and Smith) a polyhistor; Walras—training as mining engineer, mathematician, born thinker, journalism, ideas on social reform, economics chair, (according to Schumpeter) the greatest theoretical economist; Wicksell—mathematician, radical, economics chair late in life, first publication on economics at 42; Wicksteed—theologian, lecturer on Dante, first economics work (perhaps his greatest) sold only two copies.

Even as un-scholarly a scholar as myself could easily produce a far more exhaustive list than the above abridged information from Schumpeter. I have already the hunch, however, that any additions, especially those pertaining to intellectual diversity of economists, will make the case stronger rather than weaker that, by and large, great economists have been (and, implicatively, must be) more than economics specialists in their intellectual range. Naturally, I would need to know a considerably lot more than I do to be able to aver that the characteristic in question has a special relevance to economics. But it does seem (on Schumpeter’s authority, again) that the characteristic is not a necessary condition in the making of all varieties of scientists. For my present purposes, at any rate, I assume that this in fact is most likely to be the case; and I now go on to ask (and to try to answer) why should it be so.

Time was when single individuals, great minds by any definition, could apprehend and assimilate, the totality of human knowledge, largely because the totality, compared with the totality today, was very small indeed. The days of polyhistor, despite stray, and rather weak, exceptions, are gone. The present is the age of division of labour in the sciences as well as in the arts. But knowledge, fundamentally, is one and indivisible. The divisions, dictated by the

growth of knowledge, can never be complete nor the separation among the different sciences, allowing for concentration and the consequent further advance, real. And the great scientist is seldom the narrow specialist, but one who can apprehend, as far as he can, the totality of human knowledge. Given this, it is still possible to discern some very interesting differences among the various different sciences. It is easy, first, to distinguish between the formal sciences (formal logic and pure mathematics) on the one hand, and the empirical sciences, on the other. One can look for different types of differences; I myself am here concentrating on just one of these: which science can more or less do without which. I should like to begin by emphasizing that a really creative scientist (which, indeed, is the variety of scientists I shall here be mainly keeping in mind) disallows rules of creativity: rules follow him rather than vice-versa. And yet to be a really creative scientist already assumes knowledge of a lot more than what a would-be scientist might aspire to acquire; a certain minimum knowledge of scientific method as well as of his own universe of discourse—and, indeed, something also of some of the related universes of discourse. In the particular division among sciences that I have just made, it would be fair to suggest that the empirical scientist cannot expect much of himself without having a reasonably strong command over formal logic and, depending upon the demands made by his particular subject matter, pure mathematics. I cannot, with equal force, suggest that the formal logician, or the pure mathematician, would also need to possess an equally strong command over one or more of the empirical sciences, although I should consider such a command, at all events, to be of some advantage.

Among the empirical sciences themselves, the divisions may broadly relate to their being physical, biological, and (human) social. I have already spoken of the significance for these of the formal sciences. I shall, however, repeat that the nature of the particular subject matter is perhaps the decisive factor in determining the range and content of the formal sciences which the empirical scientist must needs have command over. But such a scientist can safely err on the side of knowing more, and more deeply, of the formal sciences. This can be easily asserted for the physical and, only slightly less emphatically, for the biological sciences. The lesser emphasis for the latter is no more than notional; and my only justification for making the distinction is the relatively higher level of abstraction in the former. The social sciences are in a rather different category from this standpoint. Negatively speaking, I

cannot expect any social scientist to be great who is not fairly conversant with scientific method as also with the broad methodological and theoretical contours of most of the other social sciences and of some of the physical and biological sciences. But let me first have done with the non-social sciences to be able wholly to concentrate on my special term of reference, the social sciences. It is, I think, possible for the physical, and, perhaps to a slightly lesser extent, the biological, scientist more or less to abstract from the social sciences. But I am not sure. I should imagine that there is one area where the physical as well as the biological scientist may stand to gain from an awareness of the social sciences—in the process of problem-raising and also perhaps in that of hypothesizing, the two really creative aspects of any scientific endeavour. The biological scientist would need to have a greater awareness of the social sciences for the reason that he is concerned with life and he can expect to get some insights from his knowledge of man and his social behaviour. I must, however, add that perhaps no less (and probably more) helpful, for all scientists, would be an awareness of man's intellectual activity outside the sciences proper; philosophy, literature, even interest in music and the fine arts. I should, indeed, like to go still further—sensitivity of mind, something perhaps that is inborn rather than acquired, and something, too, that is equally relevant to all varieties of scientists, formal as well as empirical. That I shall myself here be nearly ignoring this last attribute is a consequence not of any doubts that I might be entertaining about its significance but of the inadequacies of my scholarship. As already indicated, I would expect the physical scientist more than the biological scientist to abstract from the social sciences. And yet I would expect the former, in a way, to gain more than the latter from the social sciences and the other intellectual pursuits and propensities. The physical scientist may well be operating at rather dizzy heights of abstraction. But he cannot avoid being man; and just as well that he is aware of human motivations and social processes. The gap between the known and the unknown may always remain wider in the physical than in any other sciences, and the physical scientist is perhaps more needy than any other scientist so far as getting insights is concerned. The biological scientist's interest in the social sciences is rather technical; the physical scientist's is, what I might call, conceptional. It may well be that he seldom gets any conceptual insights from such far-fetched interests, but its significance cannot be measured in terms of numbers. The great scientist must at once ignore his being human, and yet be as fully human as he possibly can. A lesser scientist can ascertain truth



values whereas discovering problems and putting forward meaningful hypotheses demand of him to be already great. And it is, as already indicated, his being human and his awareness of his society and his fellowmen's other intellectual pursuits that come as great propelling motivations and influences in creativity.

Much of what I have said immediately above is applicable with no less force to the social scientist. But, of course, there are social sciences and social sciences. I confess to having as much of a biased (affectionate) predisposition towards my own particular social science, economics, as it is humanly impossible not to have. But knowing it more than any other science, social or non-social, has also about the same implications as knowing one's wife more than any other woman: I know more of its weak spots as well as its strong; but, more importantly, I know of its inscrutability. Mine, indeed, is a rather peculiar sort of a science. At times I am gladdened by its systematization which would appear to compare well with the most advanced of sciences; and then I get the feeling that it is all too ethereal to contain the badge of empiricism—it looks so close to being 'pure'; pure mathematics, say. The latter presents a problem: I need to know more of pure mathematics than I do to be able first to grasp what it is all about before I can be anything like certain to be able firmly, if necessary, to debunk it all. It is, at all events, too late to be creative or destructive in economics without a strong mathematical equipment.

But perhaps there is something more fundamental in economics which invites mathematics in; the mathematically equipped economists have always had an advantage. It relates no doubt to the ability of its subject matter to be quantified and measured, and yet, in a way, to refuse to be quantified and measured. One difficulty, of course, is that its basic measure, the money-measure, is itself inconstant. Also many of the discipline's concepts, though quantifiable in a broad sense, are basically qualitative. As a minimum, I shall speak of it as a rather eelish, slippery, ability. It is this slipperiness that may prove the death knell of the pure mathematician economist as also the living potentiality of the non-mathematician creative economist: both have room. It naturally follows that no one can beat him who is at once a great mathematician and a creative economics thinker. Only, as already implied, this is, in principle true of all varieties of scientists. Among the social sciences themselves, however, while being a mathematician is an advantage elsewhere it is a necessity in economics. Most other social sciences, moreover, do not even have a quantifiable term of reference comparable, say, to the economist's national product. It is easy for the economist to allow value judgment to colour

his theoretical constructs, but in many other social sciences it is almost unavoidable. How, for instance, to judge as between different social or political systems in political science? There can be no doubt that mathematics has a far greater ease of application in economics than in the other social sciences.

The same more or less holds for the relevance to economics of knowledge of the other sciences. The level of abstraction in economic theorizing is little short of what it is almost anywhere. The economist more than any other social scientist needs to know of the nature and broad contours of the going-on elsewhere, especially, in those sciences where the level of abstraction is approaching the esoteric. The discipline has already made good use of several of the concepts in the physical sciences, which has helped it look for the empirical inter-connections in its own universe of discourse. And I have little doubt that the scope for such importation has by no means been all exhausted. But more important than the concepts or techniques of analysis, it is the way the physical and biological sciences conceptualize their facts, the way they raise problems and hypothesize that I consider to be of greater significance to the social scientist. That all this is particularly true of economics follows from the relatively greater need and possibility that its subject matter exhibits. I am also of the view that while knowledge of economics may be an advantage for the other social scientists, knowledge of the other social sciences is of particular significance for the economist. Most other social sciences tend to abstract from economic behaviour. Economics, too, treats of non-economic variables largely as data. But the economist need to go as deep as he can into his non-economic variables in order to appreciate their true empirical linkages with the facts of his own universe. I should imagine that at least some of the other social sciences might with advantage look into the methodological and hypothesizing processes in economics as well as in the other social sciences; but I doubt very much that the problems of quantification and measurement that they face would permit a great deal of easy application.

For good or evil, mathematics has not yet done away with extended use of ordinary language in economics. It is, therefore, of some importance for the economist to have command over language. Preferably, I am sorry to have to say, over English, if only because the non-social sciences have always had much greater felicity as well as facility of inter-language communication than the social sciences. Clearly, this cannot be less true of the other social sciences; it is perhaps truer. But the economist, negatively speaking, can suffer a con-

siderable disadvantage if his exposition is non-mathematical and his literary style not sufficiently lucid and precise. If he is, potentially, great he cannot help operating at a high level of abstraction, and it makes all the difference how he puts down on paper what he has in his mind. He may indeed be simply ignored unless he is already a name in the discipline. After all, not every body can expect to be examined threadbare as, for instance, what Ricardo could really have meant.

I must now, finally (and very briefly), make a little more explicit use of my first paragraph than I have been doing. Quite a few things stand out as significant. Almost all are, in a sense, immigrants into economics. Secondly, the majority have had mathematical equipment, and, significantly, there is a noticeable positive relation (the paragraph does not give this information but it may be taken to be more or less true) between the quality of mathematical equipment and position as economist. Thirdly, expert knowledge of some other discipline preceded or went together with interest in economics. Fourthly, quite a few have had background of civil administration. Fifthly, and quite significantly, few have migrated from the other social sciences; Adam Smith is apparently an exception but it may be recalled that his prior work had included astronomy. Sixthly, most would appear to have had some degree of social commitment, even in certain cases, zeal for reform. Lastly, almost all must, by any definition, be considered intellectuals, which implies a wide range of intellectual involvement; and quite a few were intellectual giants, and these latter would have had a place in history even if they had none in economics.

Must a great economist be an immigrant into economics? We must keep in mind the age of the discipline. Adam Smith, whom the profession considers the father of economics was born in 1723 and Petty, whom Marx considered the founder, in 1623—a difference of exactly 100 years. Petty's first economics publication (1662) was 114 years older than Smith's *Wealth*. Cantillon had died (murdered) 40 years before the *Wealth*, Quesnay a couple of years, Turgot, five years, later, Bernonli, six, Beccaria, eighteen; and following the profession, Smith cannot be considered an immigrant—nor, indeed, the others just mentioned. And these are all great names in economics; including the three polyhistor. In fact, these six together are all founders of the science. Then it took time for economics to be included in the teaching curriculum, and still more time for establishing chairs; we are already in the 19th century. Hardly any of the twenty-five names here listed had, any way, had the scholastic education in eco-

nomics that produces most economists today. The question about immigration is nearly irrelevant. Not quite, admittedly; since there is the question why should these gentlemen have favoured economics. Of course, many others had favoured many other disciplines. But there are good reasons for economics to attract. The economic problem, even before Marx, had undoubtedly been a basic social—and, therefore, intellectual—term of reference. There is perhaps something of the reformer in most sensitive minds, and poverty is perhaps the most significant reaction point. In any case, not everybody could as easily take to the physical sciences as to a social science, and economics compared quite favourably with most other social sciences. It is certainly less woolly than most, and also allows for use of mathematical expertise. Those with prior mathematical background could also easily discern how any where in economics might mathematics be usefully applied. I myself do not consider mathematics to have been more than a necessary condition for what those of the twenty-five with the mathematical equipment did for economics—or for any such would-be aspirants. Indeed, I doubt very much if pure mathematicians with little exposure to empiricism can do much in economics. None of the seven points mentioned above can be totally abstracted from. Experience of civil administration may, today, appear to be not much important, but its relevance cannot be denied. A Keynes stuck on to the King's would more certainly have been altogether different; he might almost equally certainly have disallowed being here listed.

## CHAPTER 11

# Reflections on Inflation

From a non-real (monetary) phenomenon to something smacking of the unreal (a shadow of growth equilibrium) is a remarkable transition even for the rather curious theoretical system of economics. Inflation of course is the hero—or non-hero, although it is not all that easy even to say what it is. It has had almost innumerable incarnations, and all have the habit of somehow managing to stay on; and while this necessarily entails friction, all in all, they actually sustain one another. Quite handy for the discipline, too, since in times of need, when a particular appraisal (explanation) fails to clinch, another (related to another incarnation) can always be invoked; and another, by when explanation becomes a bother. The discipline (and society) is quite used to this sort of thing, the explanatory potential of economics being what it is. The special status of the phenomenon in question owes simply to the possibility of its having to stand on two stools, monetary and real. When it is entirely on the monetary stool, it may be a slight nuisance but not much of a problem; whether emanating from ease of production of gold or gold flowing in from a favourable balance of trade or the note issue spree of the authorities concerned, it has little impact on the relative prices. It may take time for the new monetary equilibrium to arrive, but everything may be assumed to go on as usual; the Say's law keeps the real things in tact. Could this have any implications for foreign trade? But I shall here in any case ignore the possible complications of foreign trade; the domestic ones are formidable enough. Could the relative prices between one monetary equilibrium and another perhaps undergo a distorting experience, after all—even get sticky or, indeed, cause further distortions? For if they could, inflation would have made for a reallocation of resources, including the allocation between consumption and saving with necessary consequences for investment; which means for national income, too. Looking at it from the opposite angle, could inflation be the effect of reallocation ensuing from non-monetary factors? For if it could, its difficulty would appear to relate

not so much to its having to stand on the two stools as to the manner in which the two stools are posited, with reference to each other. Already, then, inflation gets absorbed in (by) business cycles, although for a long while business cycles remained almost solely attractive for their curves, the turning points; and inflation somehow cannot be entirely absorbed by the curves alone. Indeed, one cannot even be sure that it can be entirely absorbed by business cycles, with the curves and all, either; unless, that is, business cycles were asked to oscillate around, not a given level but, a rate of change of national product. Inflation, then, is but part of the behaviour of national product over time. I shall take a pause.

All the while I have equated inflation with rise in prices, something that I will continue to do. But since inflation may not condescend to be equated with, snailish and imperceptible rise in prices, what rate of increase could it take to be chummy enough to be one with? Surely, the rate which is higher than the rate of change of (real) national product; the sky is the limit thereafter, but a definition has its own limits. But why this particular definition? The answer is that this is the only way in which the two stools can be perfectly well posited—the dichotomy, duality, vanishes; it won't make much difference whether we approach inflation from the real or from the non-real one. I am conscious of having missed out the unreal case, but I have already implied it. Growth equilibrium, particularly the Hicksian stuff (non-stuff?) is too absorbed with method to be bothered with empirical nuances; and even the transition from fix price to flex price is esoteric enough, in effect, to abstract from inflation's dual-stools in substance; even from our definition's monism. Far from being overwhelmed by the Hicksian growth equilibrium's capabilities, therefore, I declare the equilibrium itself as unreal; certainly, so far as my hero is concerned.

But the hero demands explanation, and the explanation, if it is but part of behaviour of national product over time, cannot be separated from growth; indeed, since economics heavily relies on equilibrium analysis for all its explanatory exercises, from growth equilibrium. It is a different matter that one conceptualization of growth equilibrium can be different from another; not a hopeless situation altogether. The choice, for insights at any rate, is wide, and even the Hicksian growth equilibrium cannot safely be lost sight of; method is not irrelevant.

How exactly is inflation related to the behaviour of national product? The relation, undoubtedly, is functionally reversible, of which the Classical monetary equilibrium (including, impliedly, the

Say's law or the Classical growth equilibrium) and the Hicksian growth equilibrium (including, impliedly, the relevant monetary equilibrium) are the two extreme, special, cases; the relation, moreover, holds both in static and dynamic settings. Equilibrium, any equilibrium, is, however, meaningful only insofar as it is a standard of reference for disequilibrium, such as, inflation, definitionally, is. It is of the utmost importance, therefore, that the conceptualization of equilibrium does not wish away disequilibrium. Price dichotomy, the independence of the money and real markets, must at all cost be avoided as an attractive but fatal snare. The two extreme cases are not necessarily snares, but they might well be; the crucial, saving, signal is whether the price dichotomy has even implicitly been invited in. What, then, is the price dichotomy-free equilibrium behaviour of national product? Constant proportional rate, which would imply a constant proportional rate of investment, and, in turn, of savings; if the determinant of national product is broken up, say, into capital and labour, constant proportional rate of capital and labour as well, and if technology is to be brought in, constant proportional rate there, too. What of prices? Easy; constant proportional rate there also. There is almost no limit to the scope of breaking up. The relative prices and the composition of national product must, in any comprehensive conceptualization, also be allowed full expression insofar as they appear as determining (as well as determined) elements. I shall myself neglect the complications, and proceed. Negative as well as zero rates all round are necessarily accommodated; and at each point of time, absolute quantities, as given by the rates (negative, zero or positive), are the term of reference. Disequilibrium necessarily implies non-equilibrium conditions.

Something rather relevant would appear to be missing: the transition from one constant proportional rate to another. One will be tempted to feel amused—to say that it's disequilibrium of course. I should myself suggest that the temptation had better be restrained—and the transition squarely faced. Not will even the so-called explosiveness of the situation do. Not, at all events in the way explosiveness is conceptualized—to blur the implications of the transition; even, indeed, to invite failure to comprehend the transition. The fact, as I see it, is that the transition does not denote disequilibrium as commonly understood; it is certainly not cyclical disequilibrium. Or, perhaps, to separate it from cyclical disequilibrium, it had better be comprehended as a non-cyclical disequilibrium. Any name would do, but let it be called trend disequilibrium, something that has the potentiality of forcing one constant proportional rate into another;

something that at all cost must not be confused with a statistical trend. I have already implied that cyclical disequilibrium is confined to a given constant proportional rate. I should like also to be understood to have implied that a trend disequilibrium need not necessarily, in fact, come out with another constant proportional rate. I must, therefore, separate the two possibilities. The trend disequilibrium which, in fact, comes out with another constant proportional rate, I shall call (for the positive case) release or (for the negative case) reversion. In other words, the trend disequilibrium has its own potential transition—to release or reversion. Historically, the transition from a zero to a positive rate (the industrial revolution or the take-off) is well understood; but it is of course little more than a special case of my conceptualization. I should myself have preferred not to treat trend disequilibrium as disequilibrium at all, especially if it is not allowed its own transition; and perhaps even if it is allowed. Let me explain. Is this special transition—to release or reversion—itsself a disequilibrium? It is (if at all a disequilibrium), undoubtedly, a rather peculiar kind of disequilibrium. It can be likened to the physiological condition (disequilibrium ?) in the person of a pregnant woman, something which a physician would, I think, hesitate to call a deviation from healthy state. Surely, the label of (healthy state-wise) equilibrium or disequilibrium is meaningful only for the period between one child-birth and another conception. True, a pregnant woman as well as a non-pregnant one has the normal risks of diseases. So has an economy passing through the transition to release (or reversion) the normal risks of cyclical disequilibrium. But the point is that the two aspects must be separated, and the non-normal case recognized. In my view, therefore, cyclical disequilibrium—the only disequilibrium that the profession recognizes—is best limited (considered relevant) to a given constant proportional rate. Following our analogy, a trend disequilibrium which fails to come out with release (or reversion) is a case of still-birth.

I am committed to be treating of prices in, fundamentally, the same way as of the other elements in the behaviour of national product, Public policy may appear to assign a special role to prices, but I am assuming that, despite the intentions of public policy, prices in their destined role as signalling servants of the competitive mechanism (no matter how the mechanism is free or constricted, except perhaps when it is totally constricted which, in any case, I am ignoring) cannot help affecting, and being affected by, the other elements in the behaviour of national product; the elements of the cosmos are [too intertwined to allow special roles. I must now return to trend



disequilibrium. What is its relevance to inflation?

The relevance, I think, is simple—and profound, although I can here only indicate the broad contours. To put it rather crudely, I would suggest that trend disequilibrium is a signal for changing the (howsoever defined) existing rate of change of national product; whether a positive or negative will depend upon what a particular trend disequilibrium, on the whole, exhibits—a more or less persistent upward or downward trend. To put it crudely, again, I would expect (define) a cyclical disequilibrium proper as a deviation from the existing rate (of change of national product) of which the tops and the bottom, tend on the whole, to cancel out around the existing rate whereas the tops and bottom in trend disequilibrium cancel out around a rate different from the existing one. I cannot tell if any definitive distinction can be made in terms of the amplitude and duration of the deviations in the two cases, except in the sense, of course, that impressions based on freak (non-persistent) amplitudinal or durational instances have particularly to be guarded against in the case of trend disequilibrium. The so-called run-away prices need, similarly, to be abstracted from any freakish instances; no economic theory—any theory—can handle freaks. And, of course, run-away prices are to be interpreted both as positive (rising) and negative (falling) behaviour; inflation cannot be grasped independently of its anti-self. I need another pause.

Trend disequilibrium deserves to be more firmly grasped. The tradition has an investment dichotomy as well—autonomous and induced. I shall follow it; indeed, I propose to embellish it—that is what I needed the pause for. Now, induced investment is given by the interaction between the multiplier and the accelerator. But does this interaction really exhaust all of what could be induced investment? I do not think so myself. The total possible induced investment is the sum of the interaction (I shall call it the indirect investment effect) and, to use the more commonly understood, Hirschman's terminology, (the sum of) the backward and forward linkage effects (I shall call it the direct investment effect). Much depends on whether the distinction between direct and indirect investment effects was genuine or spurious, which, in turn, depends on whether direct linkages between investment and investment are an empirical reality; or, in other words, whether the multiplier and the accelerator leave no investment—investment relation outside their jurisdiction. The fact is that part of the impulse generated by an autonomous investment which is (or can be) imbibed by the multiplier and the accelerator is diffused for the economy as a whole irrespective of the loca-

tion of the autonomous investment in question. It is the part of the impulse which is imbibed by industries directly (backwardly or forwardly) related to the industry where the autonomous investment in question had originated that is specific, non-diffused; something that, almost definitionally, cannot be handled by the multiplier and the accelerator. The indirect investment effect is demand (income) induced whereas the direct investment effect is demand (income) inducing (or supply induced). It is possible to define the multiplier in a manner that acquires its full value after the direct investment effect has worked itself out; and, similarly, for the accelerator. But it then becomes a matter of terminology, and, in any case, the appreciation of the relevant empirical inter-connections will have been blurring rather than illuminating. Of course, the two are related, but of this in due time. Whether a particular autonomous investment generates induced investment more through the direct or the indirect investment effect is, of course, a different matter altogether. And now I can take the jump (I have, not very elegantly, elaborated the stuff elsewhere, *The Growth Multiplier*). I aver that trend disequilibrium is more or less specific to the direct investment effect, or vice-versa. If one wants to watch, understand, trend disequilibrium, one wants to watch the direct investment effect.

What of inflation? Inflation is, of course, not specific either to cyclical or trend disequilibrium. But there can be little doubt that the more serious manifestations of inflation are to be expected in trend disequilibrium. I would go as far as to suggest that almost all historical and contemporary cases of major inflation are to be looked for in the birth pangs of economies struggling to tear off from the existing rate of change of national product, not excluding the notorious post World War I inflation of Germany and the one besetting the contemporary, so-called, developed as well as developing economics. After all, it is only natural that prices act as signals—not only for, what is commonplace, reallocative purposes but also, what is perhaps not so commonly understood, for the purposes of change in the behaviour of national product.

But if inflation demands comprehension of trend disequilibrium, the relevance of the direct investment effect is not difficult to discern. Inflation in the lap of the indirect investment effect can be naughty, but the multiplier and the accelerator are rather disciplined, and disciplining, parents; of course, the parents can themselves be turned crazy or, even if sane, ineffective. But even these possibilities, I would suggest, emanate from the direct investment effect and inflation in the arms of the direct investment effect can be petulant as well as

aggressive—and the arms, although designed to cope, are essentially accommodative rather than, generally, initiatory.

The explanation, fundamentally, relates to technological change, I should already have been taken to imply this, too. The accelerator of the indirect investment effect cannot really cope with any very significant changes in technology. In principle it might, but that would be no different from Keynes, general theory coping, in principle, with macro dynamics. Given technologies are the natural habitat of the indirect investment effect's accelerator. The accelerator itself is in the crucible in the direct investment effect, which, in due time, passes it (the emerging accelerator) on to the indirect investment effect. The crucible has many other elements as well—not excluding the multiplier; which, too, in due time, is (with its new value) passed on to the indirect investment effect. And prices. Economics needs to look as intently as it can on trend disequilibrium—and the direct investment effect. It is, I promise, an exceedingly hopeful prospect. I shall not be so rash as to suggest that the foregoing constitutes an explanation of inflation. All that I can hope to have done is to have indicated that any possible explanation of inflation (and, indeed, of economic growth) might need to take account of the distinction between indirect and direct investment effects. Actually, the first step will be fully to derive the deductive consequences of the relations I have so sketchily put forward, in order to be able both to check up the logical consistency of my formulation and to look for more of the relevant empirical inter-connections.

I might indicate the possibilities by asking a question or two. Would the nature of the behaviour of prices in a trend disequilibrium pushing for a release be different from that in a trend disequilibrium pulling for a reversion? For instance, could the former be a persistent rise, and the latter a persistent decline, in prices relative to the existing rate (to take the positive case) of growth of national product? The answer depends on the relativity of the direct and indirect investment effects: if the latter effect is the stronger, prices will be alternating between increases and decline with a bias for decline; if the former is the stronger, prices will be alternating between increase and decline with a bias for increase. The explanation is that the accelerator more or less concurrently adopts itself to the multiplier so that demand gets relatively quickly quenched, and, since the accelerator has the probability of over-reaching, demand may very likely tend, on the whole, to lag behind supply. The fluctuations (whether cyclical or trend) are but the expression of the results of decisions having belied the intentions behind the decisions. Having, however, already

associated the direct investment effect, fundamentally, with the trend disequilibrium, it is obvious that prices in trend disequilibrium will have an increasing bias. One must remember, however, that neither is trend disequilibrium free from the operation of the indirect investment effect nor is cyclical disequilibrium free from the operation of the direct investment effect; the declining bias of prices in cyclical, and the increasing bias in trend, disequilibrium will necessarily be less pronounced than would otherwise be the case. The behaviour of prices will, indeed, be a reliable indicator of whether an economy was in the grip of a cyclical or trend disequilibrium. The policy implications as related to the choice between working on the direct or indirect investment effect are also not far to seek; clearly, the choice, in the last analysis, relates to autonomous investments generating more powerful impulses for the one or the other investment effects. The choice, indeed, is available at the level of the behaviour of national product as well: the trend disequilibrium may simply, through operating on the indirect investment effect, be contained, or, the trend disequilibrium may deliberately be assisted in culminating into release (or reversion).

How is the latter to be done? Limiting myself to the positive case again, by enhancing the value of the direct investment effect (implying favouring autonomous investments with impulses biased towards direct investment effect), I would say. Would this not cause further inflation? Not necessarily. The indirect investment effect continues, of course, not only to be in operation but also, to imbibe the consequences of the direct investment effect as a possible balancing element in the behaviour of prices; and this is apart from the fact that any autonomous investment cannot but generate appropriate impulses for the indirect investment effect as well. The trend disequilibrium is indicative of the distorted relation between direct and indirect investment effects, and the distortion can only be met by what would appear to be engendering further distortion. But it is only through this that the economy can be helped to enter into another equilibrium, where equilibrium may be expressed in term of the relative constancy of the ratio of direct to indirect investment effect, or vice-versa. More fundamentally perhaps it is the relative constancy of the ratio of autonomous to induced investment which defines equilibrium, but there can be no doubt that it is the relativity between the two effects that makes induced investment what it is. The Domar model, for instance, also presents the paradox of having to run faster to be able to stay on where one (the economy) is—through the matching between the income generating and capacity creating con-

sequences of investment. My own formulation is based on a disaggregated conceptualization. Inflation, as well as growth, demands as many empirical interconnections to be caught hold of as are necessary.

## CHAPTER 12

### Revisiting the Growth Multiplier

Once upon a time I wrote a book entitled *The Growth Multiplier and A General Theory of Economic Growth*.<sup>1</sup> Some reviews, yes; but the book was more or less ignored. The *Economica* reviewer, who considered the book to be concerned with a multi-sector dynamic system and its response to certain autonomous changes, said that although the problems dealt with were essentially mathematical they were treated in an entirely literary fashion, and that it was not surprising, therefore, that none of the main conclusions was rigorously deduced and some were extremely vague. I should have, he had concluded, written down my system of equations and showed that they did yield the conclusions that I had asserted. As it is, I could not do this when I wrote the book, and I cannot do it now; I have not the necessary mathematical equipment. (Have I no business to stay in economics?) And it is already clear that there is little exciting about it all for anyone else to have tried instead. I have myself, for quite some time, itched to restate, in an entirely literary fashion again, the central theme of the book. But the futility of the job has restrained me. I would, therefore, try something different. I shall take up the provocation for writing the book, and anything of the book proper, if it did intrude, will be rather incidental. I have no patience, although it is only 160 pages or so, to read through the book, and perhaps even otherwise I would prefer rather to depend on my impressions of the book. I shall concentrate on the why of a 'general' theory of economic growth, including what such a theory could possibly illumine which the existing theories, old and new, do not.

What could a general theory of economic growth possibly mean? A theory, I should myself think, that is capable of explaining the behaviour of national product (or any variant of it) in terms, not of equilibrium alone, nor of the first vertical movement (trans-

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<sup>1</sup>, Asia Publishing House (1962).

formation or industrial revolution or take-off) alone, nor indeed the over-all process alone, but, of all these three together. This is also how the book conceptualized a general theory of economic growth. As I saw it then (and I have not progressed at all in the matter), no theory of growth, in anything like a systematic manner, singly encompassed all the three elements. There was (were ?) the Classical theory (theories ?) which concentrated on illuminating the over-all process, without specifying the attributes, or explaining the fact, of the first vertical movement. There was, of course, apart from the sinking in, or evaporating into, the Stationary State, the equilibrium given by Say's law; but this was an extension of the ever-equilibrating competitive mechanism. There was, then, the Marxian conceptualization of the process, which was, of course, an aspect of social causation, and, apart from the insights on economic fluctuations, if one must see equilibrium, it was tagged on to the movement from one stage of society to another; by the same token, it would be rather forced to look for the first vertical movement. Schumpeter's conceptualization has its own process, equilibrium and, though not any first vertical movement, several vertical movements corresponding to the several equilibria. It is all peculiarly his own in that the mechanism behind all this, innovatory push, has rather too thin a base, credit creation, and a too restricted frame of credit creation at that. Lewis concentrated on the first vertical movement, where equilibrium, as a forced interpretation, could be seen in the eventual inelasticity of labour supply, and the conceptualization of the general process, definitionally, partial. In essence, this is also, with a weakened emphasis, true of certain other transformation formulations, whether of the Rosenstien Rodan or the Hirschman species. Rostow dealt with the first vertical movement within a general and wide-sweeping (parallel to the Marxian) conceptualization of the process (and, forcedly, equilibrium) tagged on to certain stages. I am also aware of Hicks' razor (something akin to Occam's) which, in principle, excludes anything other than equilibrium to be a proper term of reference for a theory of economics growth. All in all, I do not see around a general theory of economic growth of my conceptualization.

Could it be that there is something, empirically or methodologically, illegitimate about this conceptualization? I know it said how difficult the problem of explaining the behaviour of national product over time is, how one can at best produce a, rather than the theory of growth, and the rest of it. The Hicks' razor itself was, ostensibly, a compulsion, emerging from the realization (discovery ?) that the best, and, therefore, the only, way of tackling the (empirical)

phenomenon of economic growth was to treat it as a problem of method rather than a problem of behaviour. Could there be anything neater? Unless, if I might murmur, one were not too overwhelmed to deny the neo-classical revolution—as a revolution in the explanation of behaviour. That was nearly too neat, too. Only the Hicksian revolution is, for being explicitly stated what it is, more honest. Not that empirical behaviour is ignored altogether. Indeed, it is the only thing considered—for, all ifs and buts notwithstanding, illustrative purposes; and, of course, for judging other theories—and non-theories. The Classicals' was (were) undeniably (poor Bamoul, how let down his magnificent dynamics must have felt!) static; the fix price, the flex price, and of course growth equilibrium proper; the thing. Actually, only non-theories (analytic constructs, younger consins of the neo-classical micro constuct) are, can be, the appropriate illustrations, turnpikes and—and.... In a way, economics might celebrate; we have now something corresponding to literary criticism, as a high-brow branch of economics. Everything can now fall in their places. But, of course, there is only one right place—growth equilibrium; the rest have their non-places, to rest, or grumble. The Hicks' razor, indeed, gets its inspiration from Frisch and is avowedly a method (which takes one back to Walras) rather than (directly, at any rate) such near-untouchable a thing as a theory of economic growth. Clearly, I am interpreting things in my own light and, ignoring Schumpeter's warning on the point, asking the method to have an empirical term of reference, which, I do not see how could be anything other than economic growth. Perhaps more seriously, the Hicks' razor may be said to be no more than highlighting the rather well accepted procedure of the discipline to examine economic behaviour in terms of equilibrium as an empirical as well as analytic standard of reference. After all, there is no place in micro theory for breaking up the analysis into anything like the three elements I have emphasized for a theory of growth. And, in any case, there is nothing concerning micro behaviour that is not adequately taken care of by micro theory. I find that I cannot here go as deeply as any comparison between micro and macro theory, including the implications of dynamics, would demand. But it is quite obvious that nothing similar to the first vertical movement can be said to be opposite to the micro universe. Nor can static analysis, which assumes instantaneous actions and reactions, accommodate any distinction between process and equilibrium. Disequilibrium in micro theory, moreover, cannot be as vigorous a phenomenon of study as it is in macro dynamics; apart from anything else, the objective reality of disequilibrium in the



process of growth is too visible to be likened to the (despite cobwebs) notional micro disequilibrium tagged on to the no less notional micro equilibrium.

I cannot begin to try to minimize the problem of explaining the behaviour of national product over time. Even the little that I know about the problem should be enough to make me more responsible—the problem is really complex and intricate. But, surely, I cannot be far wrong in pleading for the problem to be a little more clearly (defined, might be a strong word) apprehended than, I think, it is at the moment. What is the problem—of the behaviour of national product over time? Obviously, I am back to my conceptualization. But why ‘general’?

The position, as I see it, is that it (‘general’) is redundant; if, that is, a theory of economic growth is understood as what I have conceptualized as a general theory of economic growth. Indeed, the three facets of reality (and of the theory about these) need not be as blatantly demarcated as I have, so long as they are considered obvious. And if they are considered obvious, such a demarcation concerning what is really an overlapped, or rather entwined, multi-faceted reality (and, what must consequently, demand an integrated, systematized, explanatory hypothesis), would appear to be worse than unnecessary—pedantic. I have been a little too swift. So I had better go a few steps backwards. What is the problem?

To apprehend the problem is, of course, to apprehend the nature of the behaviour in question. The nature, undeniably, is change; economic growth is, of course, economic change. Change will give rate. But that is secondary; mathematics—arithmetic or calculus. What is, just now, primary (and, I hope, obvious) is that growth equilibrium cannot wholly absorb change in its entirety, in all its quality. Admittedly, equilibrium dynamics will let follow its shadow, disequilibrium. But change has other, real, elements, as real as (more real than ?) equilibrium. They are no one’s shadows; certainly, not of equilibrium. As a matter of fact, nor is disequilibrium anyone’s shadow.

But just as equilibrium implies disequilibrium (or disequilibrium implies equilibrium) so does change imply absence of change, in the relevant universe of discourse, at all events. The physicist may not (will not) ask what were things like before Newton’s first law became relevant—or necessary. But the economist might—indeed, cannot avoid asking—what could things be like in a state of, appropriately understood, no-change. If he didn’t, he must be blind. It is not just a matter of history; the majority of national products are, today, so

very sleepy. But even if he refused to be concerned with no-change, how could the economist possibly (legitimately) be disinterested in the first change, what I have spoken of as the first vertical movement?

What is process, then—the general or over-all process of economic growth? The Classics knew; and Rostow too. So, we should know, too. If economics decides not to touch no-change, simple reproduction or traditional society or, at any rate, some such thing, who (which science) would? And if it is not handled by the theory of economic growth, where else in the explanatory system of economics will it be? A theory of economic growth, verbal redundancy notwithstanding, has got to be general. There is room for elaboration, and by no means in respect only of the over-all process of economic growth. But if I tried I cannot manage a lot more of the nature of substance. However, my conceptualization of a general theory of economic growth implies at least two more points of some significance, and perhaps the implications will bear not being left unstated.

The first point is about the integrated nature of such a theory. To put it rather crudely, it has to be a theory, not an assemblage of basically discrete theories. The process of hypothesizing (the intellectual process of apprehension) might emerge from an all-embracing vision or it might take shape piecemeal, but the final product (which in the idiom of science is, of course, always provisional), the theory itself must be one, integrated, organic, whole. In fact, this is only natural. Theory brings out the empirical inter-connections, and if a theory imbibes (as it must) the relevant explanatory inter-connections in the appropriate universe of facts, it must eschew a piecemeal character. The decision concerning what is relevant is naturally a theory's prerogative; only if it comes to be seen as distorting relevance it runs the risk of being itself judged inadequate, even worse. The inter-connections, as pertain any behaviour, are necessarily elements of a class so that they exhibit an extensional (or, as a counterpart, intensional) relation among themselves. Consequently, as a hypothesis comprehends a layered (or, more appropriately, a progressively widening) generality, it cannot help emphasizing the commonest, most general, attributes of the inter-connections. This is why any theory is an abstraction; and the degree of abstraction is a positive function of a theory's generality. But the point is that a general theory, no less than one with an extremely limited explanatory term of reference, reflects the true inter-connections, and all the inter-connections it has found to be relevant; what is 'true', and what is 'all', is, qualitatively speaking, the same for all theories, as far as they go. One may go less far than the other, but that is reflective of something altogether different. It is,

in any case, so much more preferable for a theory not to be true enough, or all-inclusive enough, to being obviously (how absurd in the idiom of science!) true, or obviously all-embracing. The position, of course, is that only something definitionally true can imbibe the property of obviousness. And that would be formal logic or, which is basically the same, pure mathematics. I hope economics is an empirical science, and I hope economics cannot be made into an empirical as well as a formal science. Classical economics, which, of course, was essentially nothing else but a theory of economic growth, was probably a constricted theory; so was, and, understandably, more so, the class struggle conceptualization; but both were integrated—and empirical theorizing. So, it is clear, a theory, as a theory, can suffer on either of the two counts, for not being a theory and for being (essentially) definitional; integration is slightly less than meaningful in the former, and irrelevant in the latter. Naturally, Hicks' razor leaves us with the latter alone; I should like to christen it as the 'pure' theory of economic growth. Clearly, my conceptualization of a general theory fails a wee bit short of what the 'pure' theory can command.

The second point is in a way a further elaboration of the first. My book expressed the idea in terms of what it called the expansive time horizon. Let me specify. There are precursors, but I shall confine myself to Marx, Schumpeter and Rostow as illustrations of my specification. A general theory of economic growth must (strive to) encompass time (I find it hard to describe) in a more essential way than what is considered necessary to make a theory (and the corresponding empirical, reality—system) dynamic. The relevant time may be historical, so long as it expresses the present and the future as well, and I shall myself consider historical time for illustrative purposes; but I should like, in principle, to opt for the extreme, the non-historical time. Indeed, I shall want a general theory of economic growth to be non-specific to historical time and yet capable of illuminating the relevant behaviour in historical time. This is my specification, I have an assumption in mind. Some might consider it to be large. But let me make it explicit, first. The assumption is that nature and human nature are unchanging parameters. The first implies that the existing laws of behaviour of natural phenomena, and, impliedly, those of technological behaviour (that is, natural science) are not false. The second implies that man is not irrational (I do not require him to be a maximizer—all that I require is that he is not in hot chase of, incurring, pain and losses). I can, I suppose, assume the assumption to imply that any viable (therefore, lasting and, therefore, the only relevant) social and political institutions evolved by man will

imbibe the property of being non-irrational; and if they come to do otherwise they will be supplanted. According to the hypothesis that Ricardo implied, and Marx laid bare, it is the distributive aspect of the behaviour of national product that, with reference to the relevant technological horizon, acts as the catalytic agent of the supplantation. This is one illustration of a general theory, or, since it has already been noted that the first vertical movement and equilibrium do not find an explicit treatment, one aspect of a general theory. I can only quote Schumpeter's (theory) to dismiss it as an apt illustration; it does not at all imbibe this aspect of generality. But it imbibes an appearance, and I might look it through. Nothing prior to a certain already developed capitalism is included, and while a peep beyond is indicated, the particular innovatory push does not seem to be easily accommodable in the beyond. For if it were, what is the point of Schumpeter's emphasis of the sure fall of the system? I have already commented on Rostow's; its generality does not integrate, for there is little for it to integrate with.

So much for the implicit points. Could I, now that I am in a pontifical as well as judging mood, indicate, taking advantage of others' wisdom as well as folly, any broad and general contour of the relevant empirical inter-connections as might relate to a possible general theory of the behaviour of national product over time? A little too early; even if I was shame-faced enough to have the impulse a second time (Oh, my book!). I have been recently (in the last two paragraphs) ignoring the 'pure' theory, and I may, for all I reckon, need to take a slight recourse to it as well. So perhaps it would be safer first to inquire into the possibilities in that domain. There are there the illustrative empirical inter-connections and, of course, the elegant equilibrium. Both can be helpful; they might even allow being sought for some possible insights for the process and the vertical movement as well. I am by no means being terribly expectant, but one has to be an optimist, especially when one has to talk about a general theory of economic growth. I have my handicaps. If I could not construct a system of equations in or, later, about, my book, I could not possibly even in grasping, do justice to others' systems of equations; and these that I am talking about are such a lot of systems of equations. Someone has said that all that mathematical economists are sure of is their assumptions and their implications, so I should be all right, after all, if I assumed the mathematics of these systems of equations as all right, and tried to grasp the main point in a general, impressionistic, sort of way.

Growth equilibrium is defined in terms of steady-state growth,

growth at a constant proportional rate, while the definition of the conditions for steady-state growth, sure enough in terms of a corresponding matching of the rates of change of the determining variables, depends upon the range of the determining variables chosen. For instance, a matching between *ex ante* savings and investment or, which is basically the same thing, a matching between the capacity creation and income generation aspects of investment; the idea is to ensure full-capacity use of capital. If labour is explicitly introduced, the growth of labour force has to match the growth of capital : full-capacity use, again. Similarly for technical progress, which has to proceed at the matching proportional rate. Technical progress can create difficulties, depending upon how it affects the production function. It might disturb the balance between capital and labour. But, then, it can be assumed to proceed in a fashion that leaves the balance undisturbed; in other words, a neutral technical progress may be assumed (there are versions of neutrality, but that is a still more technical, or still less empirical, problem than the 'pure' theory itself, and is well tackled: there is, in any case, place for both). Then there can be such a thing as the technical progress function which solves many difficulties by postulating a relation between the rates of growth of output per head and capital per head: the very concept of the production function is dispensed with. All possible (relevant) inter-connections would appear to have been taken account of—what else remains?

I cannot help being reminded of the stationary state; is it a description of reality or an analytic construct? The range of choice is quite wide; one can conceive of as many versions as one wanted. One postulates the engine (of growth) one wants. The decisive thing is something unpleasant happening to the engine; as the engine gets stuck, the economy has no option but to get acclimatized to the plateau, the stationary state. And the stationary state also expresses a constant proportional rate, a zero rate of growth. The Classics had accumulation as the engine so, naturally, their stationary state resulted from accumulation tapering off. And growth equilibrium of the pure version would appear, except for the near unanimous vote of no confidence against Say's law, to be basically in the Classical tradition, after all; empiricism and all that.

That I sometimes tend to get a little pessimistic about (the pure) growth equilibrium, (incidentally did my book for the first time make use of the expression, growth multiplier) even as growth equilibrium, is I am sure, due to a certain metaphysical predisposition to which I am, in fits of desperado, prone to be susceptible. And whenever I have wond-

ered, I have, by a sort of reflex, thought of the very same Classics. I realise that in the soothing environs of Say's law, and circumstances, they (I exclude Malthus of course) could not possibly have realised what Keynes, and circumstances, decisively forced their progeny to realise, but the Classics represent an empiricism which appears to me to be slightly different from the one represented by growth equilibrium of the variety I have been talking about. Of course, there is a hidden assumption in the Classics' interest in the stationary state (a not very likeable state, despite J.S. Mill) as well as in the interest in steady-state (a very likeable state, despite Hicks' razor). But somehow I cannot escape the feeling that the Classics' reckoning of the stationary state flowed from their intellectual preoccupation with the processes and problems of growth whereas (and I may be dumb as well as unfair) the pure theory's reckoning of the empirical inter-connections flows from their intellectual preoccupation with the logic of steady-state. I get the impression that the latter's saving, investment, labour force, technical progress may almost well have been plants, water, gardener, grass cutting machine or, for that matter, anything. That is why I spoke of their empirical inter-connections as entering their formulations for illustrative purposes. And that is why I spoke of their theory as being pure; unstinted by genuine empiricism. The Classics, too, could more reasonably assume a more or less vigorous competitive mechanism than any present-day economic theory. There is, in principle, nothing objectionable about assuming perfect competition, or about defining equilibrium as the state of the most preferred of available alternatives. But the difficulty is that the deductive consequences of an analysis based on such assumptions turn out to be worse than lame and blind when confronted with the objective reality; they are supposed to provide a standard of comparison for, if not also to explain, the reality, and they cannot do it in any meaningful way.

I might appear to be mimicking...from ignorance. I have already owned the possibility so far as ignorance in concerned. But I find it difficult for myself to believe that my impression that the whole thing is a little contrived is totally wrong. Of course, it is a deliberate contrivance, and for a very honourable purpose, too. But I shall stake at the minimum possible. And I shall fling a potentially (for myself) suicidal bet. Does the contrivance have, directly or indirectly, more empirical content, or a more honourable purpose, than the Walrasian general equilibrium system? I do not myself expect the purists to answer in the affirmative, and I would myself agree with the answer. Now, to the best of my impression the Wal-

rasian system is no theory. There is, of course, a theory behind it. If I might look a little more backwards, I do not expect the Classics (minus Malthus; at all events, Smith and Ricardo) to be persuaded to consider their demand-supply price construct to be a theory; they have their value theory of which this construct is a mere shadow. The pure version of growth equilibrium is not only, like the Walrasian general equilibrium, a non-theory, it is, unlike the Walrasian general equilibrium, without any theory behind it. The Walrasian general equilibrium is also, I think, a more harmless standard of reference than the pure growth equilibrium; no hopes can be raised about the former while society as well as (our) science carries an impression of hope as regards growth equilibrium. After all, we are all accustomed to the predictive sterility of price theory, notwithstanding the dictum about the optimum allocation of resources. It is difficult to be satisfied with a similar prospect for growth equilibrium; it will not do to grasp that steady-state growth is a wonderful standard of reference as well as an ideal. If it is any solace, the purists, I dare say, are in excellent company. The three revolutionaries of marginalism had, I think, a value theory comparable (in the sense of being empirical) to that of the Classics'—it is an altogether different matter they had utility rather than (some version of) cost as the prime-mover, and it is also a slightly different matter that utility is a much more slippery stuff than cost. But the neo-classical theory that eventually matured (special thanks to Marshall) dissolved cost as well as utility in their own flesh and blood, by displacing them wholly and completely by their proxies, demand and supply, more effectively than ever before. Smith and Ricardo would have shuddered, and even the marginal revolutionaries could not possibly have felt entirely happy. The macro (dynamic) equilibrium theory (the 'pure' theory of economic growth) as well as the (Marshallian) neo-classical micro equilibrium theory is not only not theory, there is no theory behind either. I know that I and they are on two altogether different planets: they cannot but feel amused about my lack of comprehension, and I cannot but feel unsympathetic about their involvement. Evidently, so far as I am concerned, the pure theory does not appear to have tremendous possibilities for my purposes; surprisingly, not even on equilibrium.

I will linger a while on with the surprise. Already I find myself a little more than usually confused. The purists have diverse sources of inspiration including neo-classicism and von Neumann. But also Keynes and, by a further remove, the Classics (and not solely because of Malthus). How is it that they, while making full use of the

Keynesian tools and relations, turned out to be so unKeynesian in the sense that while Keynes (his theory) generated policy, showed predictive capability, I find little of that nature ensuing from the purists. For instance, they might have illumined and helped tackle, the problem of inflation. Could it be that in this particular area economics is barking up the wrong tree altogether? Everything has a purpose. But could it be that we are here incessantly engaged in sharpening the tools without applying them, or even not bothering whether they could be applied at all? And this particular area is the main area of the discipline. It should have been a happily angury to find interest in economic growth revived since neo-classicism had all but obliterated macro economics from economics. But what a return to Classicism—equilibrium obliterating everything else. Taking the neo-classical and the growth equilibrium purists together, one might wonder if the universe of economics would not be a more illumined universe with a little less of method and a little more of content—empirical content.

I should myself think that the matter is more serious than what a little less of method and a little more of content might indicate. The explanation for the policy barrenness of the pure theory lies in its distorted intellectual preoccupation which I earlier on contrasted with the intellectual preoccupation of the Classics—and which can be equally well contrasted with that of Keynes; indeed, with that of all the others that I have examined. Keynes as well as the Classics (and the others) struggled with an explicitly empirical problem, any policy implications flowed from their solutions. The pure theory would appear to begin with a policy orientation, and their formulations have had to fit in with their appreciation of policy. In this sense, the pure theory is, after all, not all that pure—it is tinged, throughout, with an implicit value judgment, a welfare slant. It has in this respect, an excellent forerunner, their first cousin, the neo-classical micro theory; except that the neo-classical theory tries to absorb, in its own rather tangential way, more of the real empirical factors behind demand and supply than does the pure theory of economic growth. The pure theory does not find it necessary or possible to go deep into the empirical factors (saving, investment, and the like) which it introduces as illustrative examples for the illumination of growth equilibrium. The logic does throw light on the nature and causes of disequilibrium, but one would want to know more. If a certain given coefficient of expenditure is decreed by experts for, in some sense, 'good' living, any deviations from the given pattern will, of course, indicate not good enough living. But one would also want



to know how and why one happens to experience a distortion of the given pattern of expenditure. The pure theory does not assimilate its empirical inter-connections in a manner comparable to what Keynes and the Classicals do with their own empirical interconnections.

There is another consequence of lingering on with the surprise. The pure theory has to be borrowed from in spite of itself. Part of it is old anyway. And one can be selective. It is a distress choice, but one might as well show grace. Not all growth is steady-state. Indeed, no body says so. That, however, is neither here nor there, if only because there are no bodies moving on for ever in a straight line either. But the study of steady-state is not sterile. From there on, however, a lot more than mere elaboration of the deductive consequences is called for. And here the path to follow is that shown by Keynes and the Classicals, and the others. In other words, the term of reference must be the empirical problem, not the analytical problem—of logic. No empirical science can do without deduction, but the quest is for material truth, not deduction for its own sake. Only then can the policy barrenness of the construct go. And the consequent fecundity might not be limited in its consequences to the problem of equilibrium alone. I spoke of the Walrasian system as having a theory behind it. I might speak of the pure theory of economic growth as having a theory beyond it, a theory it might help generate.

But that is hope for the (distant) tomorrow. What of today's possibilities? It does not seem easy, to-day, to approach the general theory from the side of growth equilibrium. One would, I think, need to approach it from the side either of the first vertical movement or, indeed, from that of the general process of growth. This, however, is a matter of opinion, and detail. The main thing is to aim for a general theory—as one, integrated, organic, whole. Only such integration can develop a built-in corrective mechanism which will admit in the components, and the interrelations, of the three facets of reality appropriate to the analytic (logical) and explanatory structure of a general theory.

The first vertical movement is a hard nut to crack. I may have a pupillary bias (and his construct lacks generality even on the first vertical movement) but the Lewis formulation, as far as it goes, has the merit of relatively greater operationality as well as being more illuminating compared with many other constructs on the problem. The real difficulty, and not only in respect of the first vertical movement, is that while the broad determinants are fairly well known, they keep hinting at their own determinants, and these, latter, are very likely to do likewise. No theoretical system can absorb such a

sequence; much less a measure-poor (the inconstant money-measure) discipline like economics. That is one reason why the first vertical movement as well as equilibrium tends to attract more of non-theories. The difference between the two, however, is that the constructs (models) on the former have a surfeit of crude empiricism, which no hypothesis can indiscriminately assimilate while the models on the latter, being too esoteric, are rather allergic to genuine empiricism which no hypothesis can do without.

There is a wide choice on the general, over-all, process of growth, as wide-sweeping as Marx's (and Rostow's) and as constricted as Schumpeter's, with the Classics falling in between. And there is also already much meaningful in the construction on the first vertical movement and even (so far as the definition of growth equilibrium and of the fundamental conditions of growth equilibrium are concerned) in the pure theory of economic growth. I myself tried, in my book, to assimilate the various insights (adding, in a foolhardy sort of way, my own), although, as one reviewer put it, all that was a brave failure—at best. I had tried to dissolve the dichotomy between the first vertical movement and growth equilibrium, not by wishing away but, by integrating them, as they are in real life, in a single process—and, as far as it went, in a single analytic frame. While I reckoned the first vertical movement as an empirical as well as a conceptual point of departure, I considered the movement to be a species of the genus thrown over in the general process of growth. The genus, as I nicknamed it, is "release", any point in the process of growth where a qualitative (vertical) change takes place, pushing the rate of growth to acquire, in a sustained manner, a recognizable higher value. The first vertical movement becomes a special case where the transformation is from a zero to a positive rate of growth. After all, it is only courtesy to grant the appellation, 'first', to the industrial revolution beginning in England. Can one assert that no similar vertical movements had ever been experienced by society before? In any case, my conceptualization had to take note of the possibility, since its term of reference was, what I called, the expensive time-horizon. The equilibrium, as an analytic term of reference as well as a phase, was itself conceptualized as something confined to the period between any one release and the release following it. The whole relevance of disequilibrium was, consequently, limited to the phase of equilibrium, both of, as commonly understood, cyclical disequilibrium, and of, what I termed, trend disequilibrium. The latter disequilibrium was conceived of as an abortive release (or, since I had to take care of the possibility of the opposite of release, an abortive

'reversion'). I likened the 'disequilibrium' associated with release to the physiological 'disequilibrium' in the person of a pregnant woman, adding that no physician would call the state of pregnancy a deviation from healthy state. Naturally, I had my own definitions of the concepts and conditions relevant to the corresponding theoretic structure, but there is no point of restating the stuff again in an entirely literary way. I could only add that my formulation built on the multiplier and the accelerator for one aspect of its explanatory construct, what I called the indirect investment effect. The other aspect was taken care of by what I called the direct investment effect, something akin to Hirschman's linkage effect (my own inspiration was Rostow's 1957 *Economic Journal* article, and, in any case, my direct investment effect, unlike Hirschman's, is an integrated element in my over-all theoretic system). The most crucial shortcoming of my formulation, as I saw it when I decided to publish the book and as I see it now, was the inability to provide my direct investment effect with something comparable to the multiplier and the accelerator. I did talk of the technological impact and the demand (or multiplier) impact as determining the direct investment effect, but I knew, and know, that it was, basically, an evasion. On the whole, however, the book tried to do what its own conceptualization of a general theory demanded.

But if the choice is wide, it does not mean that one just goes and buys. The difficulty is due to the problem of integration of the three facets of reality. I am myself convinced that relatively less endeavour is called for on the general process. One may indeed plumb for one already there, say, Marx's. But where to find the other two ingredients, and the sort of ingredients that fit in with the chosen third ingredient? A theory does not emerge like this. One has either to rethink (reconstruct) the theory on the general process and discover the missing ingredients, or to proceed, alternatively, with one of the other two ingredients and then search for the remaining two. Or, to put them all together, so to speak, in the crucible, and examine the possibilities, adding whatever one's light permits. This is what needs to be done, constantly, by a large number of people, people who know their business. I hope they do do. I hope they begin doing this right away. There is little justification for despondency. The crucible is far from empty. And the technical expertise already developed far from inadequate.

## CHAPTER 13

### Possibilities of Relating the Study of Religious Consciousness to the Spirit of Economic Progress\*

Given his 'feel' of the problem, what to relate to what is, of course, the prerogative of the model-builder. Even the implied limitation on the 'feel' is permissive since John, my donkeyish barber, can also presume a 'feel' about, say, the galaxies. Unfortunately, the feel as well as the consequent theoretical construct is liable to be examined as a conceptually permissible, logically consistent and operationally efficacious explanatory device. Naturally, the term 'possibilities' in the title of my paper subsums all these three attributes of the model purporting to explain the spirit of economic progress in terms of religious consciousness, unless the gentleman of the Institute who have charge-sheeted me with the topic and the gentlemen of the seminar who are to pronounce the verdict had all the time assumed that I was clever enough to take advantage of the law and assume the first two attributes away. But I would insist on total clearance, moral as well as legal.

Conceptual permissiveness is almost unlimited. After all, there is nothing wrong, on this score, even with, what I have elsewhere called, the God model, the most general theoretical construct conceived of (and conceivable) by man, which explains everything in time and space, singly or jointly, in terms of the concaet of God. And, unless one defined logical consistency in a peculiar manner, there is nothing that can bar admission of any such model on the score of the second attribute either. The fact is that the three attributes are neither independent, in that they put limits on the meaningfulness of one another, nor, singly or together, divorced from whatever the universe of discourse.

The universe of discourse here is, of course, the universe of commodities, or, to rush to arrest your sense of shock, the universe of economics. Economic progress or economic growth, to revert, is the progress or growth of commodities, *i. e.*, the rate of growth of

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\*Paper submitted to the Seminar arranged by the Indian Institute of Advanced Study, Simla in October 1969.

commodities, which, expressed in terms of the common denominator, money, the gentlemen of economics call the rate of growth of national product. I would not bother, or bother you with, whether economic progress, thus defined, has a spirit. I should myself, however, be more at home with the spirit of commodities than of men (or, good Lord, women); also I will have, in the bargain, found for my idiosyncrasy the company of my change-sheeters as well as of the gossipstarters led by Weber and of the self-appointed agent provocateur, Marx.

The universe of economics, like the universe of any science, is no more than a sub-universe, and forms part of the universe of facts; where 'fact' is defined as anything that there is or can be conceived of; where the universe of facts as well as the sub-universes keeps, or can keep, on changing; and where the boundaries of a sub-universe are demarcated by considerations such as similarities between facts. The selective function of science conceived of as a sub-universe is, however, subservient to, or at any rate the first step in, its basic, explanatory, function. Explanation, which is but another name of meaningful relationships, is, in turn developed through interlinked models such that different sciences are no more than different chains of models. It is already implied that the doors of a science are open for facts which become necessary for the explanation of facts already included in the sub-universe; and it is not particularly illuminating to begin to quarrel over naming the new facts as external or internal. It is important to mention this because economists often talk of non-economic as against economic facts. The only tenable position is that facts are either relevant or they are simply irrelevant. And what is irrelevant (relevant) is what can (not) be done without.

Now, is religious consciousness irrelevant to the explanation of the spirit of economic progress? I should hate to be forced to try to answer the question. But the way I have chosen to expose the problem of my paper signalled suicide at the very beginning. Death-wish perhaps, but let me explain and give up trying to conceal my professional secret—the professional secret of the economist, that is.

Economists, perhaps more of the growth species than the self-christend 'purists', consider population growth, capital growth and technological growth to be the three basic factors determining economic growth. Some realise, most do not, how much has been admitted. One of the former, my teacher Arthur Lewis, tried to soothe feelings by making a distinction between proximate causes and causes of the proximate causes of economic growth. But I am not sure if this characterization really makes proximate causes more, and

causes of the proximate causes less relevant determinants of economic growth. To say that the more relevant cause of the death of the man who on being rejected by his fiancée, jumped over in front of a fast-running engine, was the engine is a poor explanation of the guy's death. One might as well say that his death was most relevantly caused by his heart having ceased to function. The fact is that while the three factors, through a functional relationship, determine economic growth, they are, each of them, themselves determined (I need not spell out) by the so-called non-economic factors. In the vocabulary of this paper, the most relevant causes of economic growth are (this is the professional secret) the most irrelevant ones.

Religious consciousness is to be looked at as one of the relevant irrelevants. The 'how' of this demands a simple exercise in asking a chain of questions on what determines each of the three factors, and relating the answers to different religions, or religion as such, conditioning the consciousness of the laity. I do not here propose to engage in this exercise but my exercises elsewhere tell me that religious consciousness has an assured place among the 'determinants of the determinants' of economic growth.

But the determinants mentioned above do not by any means complete the list. I should like to mention only one of the missing: the urge to live well materially. I would further suggest that insofar as it is a basic, if not the basic, determinants of economic growth, its possible links with religious consciousness call for thorough examination (again not to be attempted here). I would myself take the position that such an urge is peculiarly related to religious consciousness, whether or not I am allowed to include "ismic" (*i. e.*, related to communism, socialism etc.) consciousness in religious consciousness.

Religious consciousness must, in the last analysis, be viewed as one of the mainsprings of social action, of which economic action is but one part. The mainsprings of social action are, in turn, the socialized mainsprings of individual human action. These mainsprings are basically instincts, which are inclusive of stored up experiences consciously or sub-consciously formulated as propelling guide-lines for subsequent action. Indeed, human instincts, love, hate, power, jealousy, compassion etc., are animal instincts. Religion, which unfortunately is a purely human preoccupation, channels and sublimates such human instincts internally, *i. e.*, for its followers. Not all instincts are, however, effectively sublimated by religion, and these continue, side by side, to operate as independent mainsprings of individual and social action. Religion thus socialises the mainsprings at

the second layer as it were.

Now, religious consciousness in relation to the spirit of economic progress must, to be meaningful, refer to those active attributes of religious injunctions and directives of which the laity is consciously aware and which are relevant, as propelling guide-lines, to economic action.

It is easy to see that not all economic action is action leading to economic progress. The spirit of economic progress defines the attribute which, when transformed into the corresponding economic action, are conducive to economic progress, and may, for instance, be likened to the Weberian conception of the spirit of capitalism. The economist's conception of the spirit of economic progress corresponds, in essence, to the three determinants mentioned above, *viz.*, population growth, capital growth, and technological growth, such that, like the relationship between the Supreme Soul and the individual souls, these three are encompassed by, and submerged in, the more fundamental determinant, *viz.*, the urge to live well materially.

Now, religion, except that of the 'ismic' variety, is seldom shameless enough explicitly to drum about the virtues of living well materially. But both when its injunctions and directives are conducive and when they are detrimental to the urge to live well materially, religion imparts consciousness which has unambiguous relevance to the spirit of economic progress.

In sum, religious consciousness is related to the spirit of economic progress inasmuch as it strengthens or weakens the urge to live well materially, which, in turn, directly touches upon the attributes transformable into appropriate economic action. The urge also operates indirectly inasmuch as it has conditioning influence on population growth, capital growth and technological growth. But religious consciousness is related to the spirit of economic progress through linkages other than its conditioning of the urge to live well materially. And these other linkages, again, operate both directly and indirectly. The analyst cannot complain: he has more data than he can expect to handle.

It is the model-builder who faces difficulties, mainly because his model fights shy of becoming operationally efficacious. Measurement is his real problem, and almost insoluble. My professional secret is, after all, a distress secret: there is a point in declaring things as non-economic if you cannot handle them as you normally handle your stuff. The economist knows what he is missing, but is he really

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to blame if he cries: Well, I know this damned thing is acutaly relevant but I can't proceed unless I declare it irrelevant. The secret is also honest!



## CHAPTER 14

### Loomises on Marx\*

Loomises' paper attempts to analyse the scientific and the unscientific strains in Marx's writings. This is done in terms of a model.

I am afraid, I have ignored the model both because I found it more cumbersome than helpful and because, on a first reading, I knew I was not going to touch upon more than a few of the points raised in the paper. I must confess that I did not find the paper as provocative as I had expected. The following comments deal with the nature of scientific inquiry and the question of methodology, which together will help me bring out, just by way of examples, what I consider to be Loomises' failure to present the correct Marxian position. I should have very much liked also to point out certain instances of confusion between Marxism and the Soviet practice and examine the so-called relationship between Marxism and Indian Materialism. I could not do this for want of time, but what I have said will, I hope, provide some basis for discussion.

#### The nature of scientific inquiry

Scientific inquiry flows from man's inherent curiosity and self-interest (including social interest) where the two elements act and react on each other. It is not inconceivable that an inquirer was guided solely by his curiosity. And there is so much in nature and society to continue to make man curious. On the other hand, even if it were possible, a complete lack of self interest must weaken curiosity. The range of self interest itself is almost indefinable. I come

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\*The National Institute of Community Development, Hyderabad (India) organised a seminar in 1966 with Charles P. and Zona K. Loomis's *Religion as a Facilitating or Inhibiting Factor in Social and Cultural Change—Notes from Max Weber Concerning India and Ascetic Protestantism* as the key paper for discussion. The paper had been circulated among seminar members in advance and their critiques (like the present one) were to be published together with the key paper after the seminar.

across a man who is bitterly crying. I am curious. I begin to find out the reason. And I may conclude my inquiry without any trace of emotional involvement. But my curiosity may become tinged with a certain amount of emotional involvement. I may react as a human being. I may generalize the condition of the crying man as a social phenomenon. I may get interested in discovering the cause of this phenomenon. This emotional involvement merely expresses some satisfaction, fulfilment, within me and this is so because, given my sensitivity, I am one of my society.

Does such involvement vitiate scientific inquiry? An inquiry based on pure curiosity does not by itself become immune to being unscientific. That is a matter of open-mindedness and method of inquiry. Marx was curious. He was also "interested". He wanted to know the 'why' of misery. He was also interested in abolishing misery. The question is whether, leaving aside his method, Marx was open-minded enough.

This is not easy to establish. But let us go a little into this question of open-mindedness. Roughly speaking, there are two alternative ways of embarking upon an inquiry. One may go into the facts of a situation, analyse them and arrive at a conclusion. On the other hand, one may have a hunch to begin with and then go into the facts with a view to verifying the hunch. Of course, the hunch itself is the outcome of a general, almost philosophic awareness of the facts. But the distinction is very much there. On the face of it, it would appear that the former is a superior scientific procedure. But most of the fundamental discoveries have followed from the latter. That Marx may already have had a hunch before he embarked upon his inquiry does not automatically establish his lack of open-mindedness.

### **The question of methodology**

Given the impelling force, the methodology used is of crucial importance to scientific inquiry. If the objective is to explain, the problem of choice of the variables impinging upon the behaviour of the phenomenon under study is inescapable. This is what is model building. One must abstract from reality. The chosen elements are a function of discrimination, judgment, and even personal idiosyncrasy. Only then can meaningful relationships be established for the purposes of deeper examination. I have spoken above of scientific inquiry flowing from curiosity and self interest. Of course, this is an abstraction. Loomises' paper proceeds within the framework of a method. An abstraction again. This is inevitable. There are two levels at which one can examine an abstraction—a model. One must,

first, take the abstraction as given and confine oneself to ascertaining the internal consistency of the model: that is, whether the succeeding steps follow logically from the preceding ones. It is only after this has been done that one may begin to examine the nature of the abstraction itself—the external consistency, so to say. For instance, one might ascertain whether some crucial elements have not been left out or some not very relevant ones included. The reason why I categorically define “which first” is that whereas internal inconsistency cuts at the root of logical procedure, external inconsistency does not necessarily do so. For the problem of choice is more of a philosophic than of a logical nature. To speak of an absolute consistency here is almost a contradiction in terms. Internal consistency, on the other hand, is a minimum necessity.

External consistency has also to be judged in terms of the set of assumptions which must be made for any abstraction or theoretical model to be workable for the purposes of analysis. When the economist says that a business firm wants to maximise its profits, he is making such an assumption. Of course, it can be, and it has been, asked whether a business firm is altogether free from non-pecuniary motivation. This is an example of questioning the external consistency of one type. I would call this “contentual” external inconsistency. The distinguishing characteristic here, of course, is that the assumption in question relates to a “fact” or reality; the assumption is generally based on the findings of another area of inquiry or discipline. To take another example from economics, the economist assumes that as one goes on eating one apple after another within a given time, the satisfaction tends to diminish after a certain number of apples have been eaten. Obviously, this assumption is based on findings outside the economist’s area of inquiry. There may also be a set of “technical”, as against contentual, assumptions. Here again the assumptions are generally borrowed from other disciplines. When the economist makes use of difference equations he is relying on the mathematician.

Both such assumptions stand or fall according as the disciplines from which these have been borrowed retain, modify or reject them. It is not impossible that such assumptions may get modified even while being applied; but the point is that, following our examples in economics, the economist here is, to that extent, not an economist.

One word on the nature of logical consistency itself. Let us imagine a religion which prescribes that punishment is given by God for various possible sins in a graded ascending (or descending) order. Very likely, there will be certain rules of the game which provide the base for sin-punishment relationship. It follows that anything which deviates from the prescribed rules will be betraying “logical inconsis-

tency". The point is that the conception of what is logical is inherently related to something more primary than what is commonly appreciated. We may call it logical cosmology. The implication is that every cosmology has its own logic. An ignorant, superstitious, man would not frown when reminded of the punishment that lay in store for him on account of a particular sin committed by him. To call it illogical betrays too restrictive a conception of logic. What is scientific is altogether different from what is logical. What is scientific is a cosmology which has its own rules of logic. In this strictly philosophic, or methodological, sense, scientific cosmology is neither inferior nor superior to religious cosmology. It is when the cosmology is given that we can say that logic is the same for everyone.

But there is a snag even here. Given scientific cosmology, the explanation of social phenomena is vastly more complicated than that of physical phenomena. The pitfalls in the explanation of social change may be due to bias. But they may be due to something inherent in social behaviour. No wonder several "isms" coexist; scientific cosmology is capable of accommodating several sub-cosmologies, including Marxism. This must be kept in mind in examining Marx's examination of social change.

### **Marxian position**

It was not pure curiosity which impelled Marx to embark upon his inquiry. He was interested. Indeed, he was a participant in the movement which flowed and gathered strength from his inquiry. The misery engulfing the masses provoked him. His scientific attitude, which was a product of his times, spurred him on to believe that it was a phenomenon which could be explained. His materialistic philosophical outlook rejected the notion of any super-natural intervening in man's affairs, and thus gave him the faith in human competence to manage his own affairs. Dialectical materialism provided him with a clue as well as an intellectual instrument to discern and explain the logic, the sequence, and the fundamental relations of social change. The emerging pattern of industrialisation worked for him as a crude though living laboratory. It was here that he tried to substantiate his Labour Theory of Value, discover the rationale of the falling rate of profit, bring out the fact of the reserve army of labour, of concentration of capital, of increasing misery of labour; and uniting all these with the other elements of his intellectual system (Dialectical Materialism etc.) he concluded the inevitability of the collapse of Capitalism. To spell out a little, Marx, as is well-known, explained social change in terms of an inherent contradiction between

the "mode of production" and the "relations of production" where, as time passes, the latter fails to keep pace with the former and, in consequence, acts as a brake on development, and has ultimately to be done away with. Since a particular class of society has to bear the main brunt of this brake, the instrument of this change is class-struggle. The inevitability, "necessity", of Communism as the next phase is as logical as any explanation and prediction of social behaviour can be. Marxian Communism is not utopian because, *given the Marxian Cosmology*, the collapse of Capitalism has conclusively been brought out and because Communism shown to be the only logical next phase. The relations of production begin to act as a brake because certain classes have vested interests in continuing them. Once classes are done away with there will be no vested interests and the relations of production will cease to be a factor in social evolution. So if you want to ensure unhindered progress of society you have to have a classless society. It is in this sense that Communism is the logical next phase. But this next phase, while almost inevitable, was not inevitable. Hence the need for hammering the point, organising social movement, propaganda and so on.

In view of this and of what I have said above, the two strains in Marxian writings to which Loomises refer appear to me to be more proportionately related to each other than what they would admit.

Let us take Loomises' treatment of Marx's dialectics. I suggest that Marx's dialectics have to be understood in terms of what I have called External Consistency. This is simply an assumption derived from Hegel or, may be, the Greeks. Marx is supposed to have modified the Hegelian form but it continues to be the assumption of the type I have mentioned above. I am not at all suggesting that everything is all right with this dialectics. But what I am suggesting is that it is not fair to confuse the question of Internal Consistency of a model with that of its External Consistency.

Incidentally, I confess, I do not fully appreciate Loomises' disagreement with the view that science must be formulated only from "material" concepts. Surely, if even mathematical infinity can, in terms of this view, be considered to be taken from reality, the definition of material concepts does not appear to be all that restrictive unless, of course, one is bent upon making the definition wide enough to embrace "stupidities and absurdities." I wish Loomises have elaborated upon specific "ideal types" which one could proceed to reject or accept in terms of material concepts and thus bring out the deficiencies of the view.

Lastly, even granted that it is a wholly correct presentation, I

do not find "that man influences the world he lives in. . . , and that man's activity is the immutable working out of a universal plan. . . ." all that devastating. I have already explained why, given the attributes of the Marxian system, the collapse of Capitalism is inevitable, and Communism the next phase. I know that the proviso "given the Marxian system" is too demanding. But there is no other way of evaluating any model of social change. Nor of evolving one.

By the way, Loomises' presentation of Marx's (as well as Adam Smith's and David Ricardo's) position regarding the falling rate of profit is, of course, wrong. In Smith, the rate of profit falls because of competition among capitalists or, in modern terminology, because of relative lack of investment opportunities. In Ricardo, the rate falls because of the law of diminishing returns. In Marx, the rate falls because of the tendency for the organic composition of capital to rise. Actually, Marx is unconvincing, even wrong here. And this in terms of what I have called Internal Logical Consistency. Quite a slip, but that, right here, is a different matter altogether.

## CHAPTER 15

### Loomises on Max Weber\*

The difference between the answer to the question "What was the cause of his death?" in respect of A, who was a successful target of a keen bullet and of B, who was a chronic anaemic, frequent victim of malaria, dyspeptic, heart patient, and a recent catch on tuberculosis, illustrates, in an important sense, the limitations of causation as a system of explanation. The limitations reach near-absurd proportions when the effect of which the cause is sought for is not something as absolutely defined as death but, say, (B's) weakness. For despite Weber's best efforts "rational organization" leaves much which is and, for no fault of Weber's, must remain less than absolutely defined. The fact is that Weber's model is not a model but a rough approximation: there are too many loose ends, and internal consistency is distressingly wanting. But, of course, it does permit Weber to develop his argument within a framework and, for our present purposes, we may let it pass. In any case, there is no question that Weber's analysis of Protestant ethic as a causal factor gave a new slant to the study of socio-economic causation which is altogether refreshing. And Loomises' attempt, in this paper, at once to weave together and contrast Weber's often penetrating, though sometimes off the mark, comments on the Indian (Hindu) society and Weber's analysis of ascetic Protestantism is welcome if only because it should irritate Indian scholars into taking up a thorough examination of the questions raised. The results of such an examination may be expected to provide useful hints to the policy makers at the Indian

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\*The National Institute of Community Development, Hyderabad (India) organized a seminar in 1966 with Charles P. and Zona K. Loomis's *Religion as a Facilitating or Inhibiting Factor in Social and Cultural Change-Notes from Max Weber concerning India and Ascetic Protestantism* as the key paper for discussion. The paper had been circulated among seminar members in advance and their critiques (like the present one) were to be published together with the key paper after the seminar.

Planning Commission. My own comments are directed at *i*) reasserting one or two of my own conclusions reached elsewhere (Hinduism and Economic Growth)<sup>1</sup> but not included in this paper *ii*) affirming some of Weber's/Loomises' insights and *iii*) picking holes in some others of their interpretations.

One of the most unambiguous examples of religion anywhere at a factor in economic life has been the practice of non-remarriage of certain higher caste Hindus in that it affects the quality as well as the quantity of the working force. There are more ways than one in which the caste system has similarly been operative as a causal factor. First, it fails to ensure adjustments in labour supply of different types to meet the new demands caused by changes in techniques of production, in organization, in resource-availability etc. Secondly, it has a disincentive effect on the higher as well as the lower castes in that there is normally a floor to the fall in the social status of the higher castes and a ceiling to the rise in that of the lower ones. Thirdly, by tending to confine major business and investment participation to only one of the castes, the system narrows the door against not improbable business genius elsewhere. While the law of 'Karma', conjoined with the belief in rebirth, leaves the theoretical possibility of doing one's best in this life to be able to better the next, it nevertheless, to the extent the law is operative, distorts the urge and effort in the present life insofar as the present life has for the normal human being a premium over the next.

As I have tried to show in my *Hinduism and Economic Growth*, asceticism is not wanting in Hinduism. What, however, does seem to be wanting in Hinduism is the concept of the "calling". I must, however, hasten to add that, in theory, the concept is there. 'Dharma' denotes excellence in this world as well as beatitude in the other. I would nevertheless admit that the former attribute of Dharma has for long somehow got too blurred to register an impelling impact on the Hindu society. This together with the other-worldly bias in Hinduism cuts at the root of the process of saving-productive investment-income chain reaction, which, I think, is the central point of Weber's analysis of Protestantism as a causal factor in economic life. Loomises' elaboration of the point by linking up the main issue with the several major and minor traits in Hinduism is ingenious and fascinating.

And now to my differences. It is curious that, while they have attended so much to Marx, Weberians have not given proper attention to Schumpeter's view, which interestingly is also opposed, as a

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1. Oxford University Press (1962).



theoretical model, to Marx's model, that bureaucratization which is so much a part of Weber's "rational organization" may indeed be one of the main causes of the collapse of capitalism. Of course, there can be too much even of a good thing. But there is little in Weber's model to take care of this development. There may also be differences in the concept of bureaucratization, especially as Schumpeter had in mind what is now known as the 'managerial revolution' Yet my main point would remain.

Loomises' generalization regarding the relative lack of mobility in Hinduism must account for the now nostalgic exploits of the Hindus in the South East Asian Islands. The famous Hindu aversion to the sea must surely be a lately developed inhibition. There is then the concept, and fairly prevalent practice, of the 'Tirthas' (religious voyages) which has the effect of at least temporarily inducing mobility as well as widening mental horizon and promoting national integration. Also the relative ease and spontaneity with which the Hindus, in contrast to the Muslims, adapted themselves to the changed conditions resulting from the British conquest of India, particularly in the field of education, leaves a question-mark which the Weber/Loomis model fails to answer.

The influence of the joint family has also, in my opinion, been unduly exaggerated. The fact that industrialization has been much more effective in weakening the joint family than the caste system is indicative of something highly entrenched and deeprooted in the latter.

I am also a little uneasy about the use made of the distinction between the ethical and exemplary prophets. Hinduism has a system of incarnations—God himself coming to the world—not of prophets. What such incarnations achieve is, it is true, the restoration of the status-quo ante. The Hindu religious leaders also appear to be doing the same. Nothing forward moving. But what is this status-quo ante? The restoration of truth, fellowfeeling, compassion etc. can have nothing unprogressive about it. The question of a norm or command to change is irrelevant since nothing can replace such eternal values of society as truth, etc. There cannot be many instances of incarnations or religious leaders very much taken up with the problem of the infringement of, say, the caste system. The reason, of course, is that the system has been pretty well entrenched, although the historical fact of the absorption of various invading tribes into the Hindu caste fold must always be kept in mind.

The caste system is, beyond question, oppressive, and denies to the Hindu society the full participation of the lower castes. The needs

of a preindustrial society demanded an adequate supply of menial and supposedly 'low' craft workers. Too bad for the latter that they did not, as members of the Hindu society, belong to the higher castes. But the society itself lived on. That is, it did not collapse as many other societies did; for instance, those based, more or less for the same social purpose, on Slavery. And Hinduism has always been free from Slavery: not even the lowest caste Hindus, not even the untouchables have ever been slaves as known to the many other historical social systems. I am convinced that the damaging economic implication of the caste system would not be obvious, or even all that serious, except in the context of the challenges, demands and potentialities of the techniques of production thrown up in the wake of Industrial Revolution. The purely human considerations are too weighty to be ignored, but we are here talking of something else.

Then, is it correct to assert, as Weber/Loomis thesis does, that the Hindus have an inherent lack of empiricism? Advances, some path-breaking and fundamental, made by the ancient Hindus in at least certain fields is admitted: Mathematics, for one. One would easily be persuaded to admire the achievements in medical science. Much is also made of the practice of oral as against written media of communication. This is, of course, confusing cause with effect. The Vedas are called 'Shruti' which underlines the oral medium. But this was a necessity: apart from the knowledge of the art of writing there was, in later years, the problem of evolving convenient materials needed for writing. Too much, again, is made in this paper of verbose and ornamental Hindu writings. This is amazing: no people in the world has evolved as concentrated and economical use of language as is commonplace in the 'Sutra' method of expression. Indeed, such an extreme had been reached that the problem later became one of interpretation: discovering what was meant in the first place. The grammarian considered himself to have been blessed with a son if he was able to save even half a syllable. Relative lack of historical writing appears in this area to be the most established of the inadequacies. But it remains to be established if this is, in any way, to be ascribed to the Hindu mind or temperament.

Lastly, it is almost naive to treat Hinduism as one entity. For instance, the economic implications of the 'Dayabhag' School of Hindu Law are vastly different from those of the 'Mitakshara' School.

## CHAPTER 16

### Re-Viewing Hinduism and Economic Growth

I cannot say why but I cannot read through my old books, and these are just enough to become plural. And since it would not be fair to read here and there, I would, for my present purposes, wholly depend on my impressions of my book, *Hinduism And Economic Growth*.<sup>1</sup> The book, moreover, did not look at independent India, and I might here concentrate on the recent rather than the distant past; and even look a little beyond.

One of the hypotheses the book put forward related to the adverse impact on economic growth of the other-worldly motivated abstinent propensity which since around the Upanishadic period replaced the robust optimism of the rather earthly motivations of the Vedic period. In contrast to what Weber had discovered in the Protestant ethic (the concept of the calling making for a this-worldly bias, and abstinence, asceticism, making for savings, which together released a chain reaction of saving-productive investment-enlarged production), the hypothesis in question postulated a weak propensity to seek material advance making for weak productive effort, resulting in constricted production, leaving room for limited savings much of which was allocated to non-productive activities, and so on. Of course, an indigenous religion is the product of a country's geography and history, and while geography has a slow cumulative impact history can spring surprises. The book in its own, rather superficial, way tried to ascertain how history persuaded or forced one or more of the tenets of Hinduism to be reformed, with what impact on economic life. That Hinduism should have (had) influenced economic life is indubitable. Like any widely believed religion (and it would not make any difference if it is described as a way of life), Hinduism affects population growth, capital formation, inventiveness and technological growth, and socio-attitudinal set-up, the four factors which in a functional relationship, as it were, deter-

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<sup>1</sup>Oxford University Press (1962).

mine the process of economic growth. Also, like any other religion, Hinduism affects perhaps the most basic of all factors in economic growth, the urge to live well, something that follows from the balance a religion strikes between this-and other-worldly motivations; and the book's hypothesis referred to one such balance at a certain historical stage. But unlike most other religions, and despite (or together with) engendering such social rigidities as those enshrined in the institution of the caste system, Hinduism has been imperceptibly ruthless in shaping the whole character of its laity, in determining the definitive attribute of the mental make-up, the psychology. It would be absurd to suggest that there are, or have been, no differences among regions or castes or classes, but there is something, further and further history notwithstanding, which as a residue continues to be imbibed by all. The differences, in any case, relate to little else but differences in the degree and duration of impact of geography, history and, more relevantly, the particular facets of Hinduism emphasised: which is why even those Indians who had never been, or no longer are, Hindus could not help having partially imbibed the attribute. And, of course, nothing can be more absurd than to suggest that the definitive attribute, the psychology, of the Hindus would have made them into human beings different from the rest of the humanity; but I would stick to my generalization as a sufficiently distinguishable property. What is it? In what sense, as I would seem to have implied, is it more fundamental than the book's hypothesis—more fundamental for my universe of discourse, economic growth? It is in terms of these that I should like to re-view my book, or, as is already apparent, a certain aspect of it. And for a certain private reason, some of my assumptions (premises) will remain implicit, and some of the implications unstated. A specimen of the definitive attribute? I cannot tell; but, of course, I am, in the literal sense, a born Hindu.

I would, on the whole, expect India's geography to have inculcated a sense of insecurity and frustration, history a feeling of humiliation and suppressed ego, the two together a vague realization of a let-down, all the more excruciating for the uncertainty between whether one has been let down or one has let oneself down, and Hinduism a sub-conscious unaware awareness of illusion. The illusion itself was a sublimation, which however, in curing the Hindu mind of a rather inhibiting predisposition, submerged it into the most incurable of mental states, moral indecision. That this particular form of sublimation involved a contradiction, in that what is moral is, almost definitionally, choice, is indicative of the same very attribute of the Hindu mind that I have in mind. After all, Hinduism was

made into a body of tenets and practices by none other than the Hindus themselves; it was an indigenous religion. A foreign commentator has spoken of the Hindu mind (or was it the Indian mind?) as being intricate. (I suppose only a non-Hindu, or perhaps only a non-Indian, could say that, inasmuch as it is difficult to be perceptive enough about oneself.) I would hate to have to get involved in defining somebody else's terms; although I confess that the all too cryptic description made me think, right from the moment (much after the publication of my book) I read it years ago. But let me return to the illusion, and the sublimation, and the contradiction—and the attribute. Indeed, first, to geography, history—and Hinduism.

Geography has been unkind in several lands, and India's geography has by no means been unkind altogether. Indeed, insecurity implies experience of security; and frustration, hope, even expectation—something that a totally unkind geography can not generate, and all that it could generate would be a certain minimum level of mental balance and physical existence. The monsoon may be, is, a more or less Indian peculiarity. But so long as the pastoral ingredient of production-consumption was a proportionately significant quantity the monsoon remains a significant but by no means decisive factor in conditioning attitudes. Its failure could be bad enough, but not too bad for most regions and the majority. The cattle and the like had the rivers and natural vegetation, and the people correspondingly not left totally in the lurch. When, however, farming came largely to supplant the pastoral ingredient as a substantial item of food, together with the implications of the progressively extending extensive margin, population growth, and a more or less unchanging technical know-how, the monsoon became the decisive influence on the mental and physical well being of the populace, which, unfortunately, it continues to be. Expecting an excellent harvest within weeks and then having to see the crop, either suddenly by floods or slowly by drought, gone is an experience whose psychologically shattering impact is obvious. So, or nearly so, is the onerousness of having to make the choice obvious; how to live and the compulsion of living. And there is no knowing what might happen the next year, or the year after. A case of unbounded optimism, but like the ball-throw on a wall the pessimism of the reverse journey is exactly proportional to the initiatory force. Only intense optimism can engender intense pessimism. It is possible to enter into details of time, of area, of population distribution, and so on, but my purpose is nearly adequately served by a suggestive appraisal.

Which is also true of history; except that history dovetails with

the foregoing on geography, if only because India's geography has more than usually made its history. Why should, after all, have peoples from across the frontiers felt attracted to this land—and fought among themselves as well as with the, continually redefined, natives? The Hindus, to be sure, had their own experience of crossing their own frontiers, but the purpose as well as the mechanism of such enterprises was rather different, something, in any case, that, for the people and the area involved (South India) must surely have proved an invigourating antidote to any debilitating consequences of geography, history, or Hinduism. But perhaps a more realistic interpretation of the situation would be that the period in question was still quite far away from what history had yet to unfold; the locale and the people concerned never earlier or since as much a victim of history as their northern counterparts have been, and Hinduism, in any case had yet, after having absorbed the lessons of the Buddhist and Jain episodes, fully to complete the process of sublimation. It was the history of the period since that is overwhelming; humiliation and suppressed ego, and all that.

Hinduism took time, but sure enough eventually cristalized its basic sublimating tenets. Variety was of the essence of the matter since it had to cater to variety, horizontally and vertically. But the cycle of rebirth as the cosmic fact of existence, release from the cycle the ultimate goal, and the supremacy of the law of Karma in between, cut across all variety. It is academic, if not sheer evasion, to argue about who among, and how seriously, the Hindus believed in these. When reason gets sublimated it is no different from instinct, and the Hindus have drunk too deep into the stuff to need to believe, to be conscious of what is part of their self. The high point of the sublimation was, of course, illusion. It was, indeed, a double illusion; the doctrine that the universe was little else but illusion, and the rather unconsciously compulsive, even faintly conscious, near eager (like that of the sinking man's) acceptance of the illusion. Such a profound revolution in thinking (?) can occur only when the will has been all but destroyed, personality turned impersonal, courage retired. But how contradictory again? The man, Shankar, who put forward the illusion thesis in much sharper relief than ever before was courage personified, an enviable personality by the most honourable standards, and nothing if not a man of will. But, then, one cannot eat the cake and keep it, too. It's no use engaging in a philosophic discussion, as Shankar was always eager to, of what had been meant; the meaning that was taken in by the masses was the meaning that was wanted : relevant to how to live with the undecid-

ed let-down. And the relevant meaning was : does it all matter, after all...all that, any way ? It is all so transitory, so superficial. Meaninglessness of existence, in a word, was the meaning that was appropriate to the mood and the occasion. The mood and the occasion remain basically unchanged. So, almost necessarily, does the Hindu mind. How depressing...but I must continue.

The best possible decision was indecision. There was little to decide about the laws of nature, scarcely more about geography, and, apparently, history was no different. His cosmic insignificance was matched by his existential insignificance. In a passive sense, the Gita's precept of detached action was as much an expression of reality as a call. (The same holds for Shankar's thesis.) The Hindu is already a practitioner...more than anyone else, at all events. In a passive sense, though; there is, always has been, so much for him in his social universe to resent, to react to. But this is a case of non-reaction. His urge has, not a ceiling but, a curtain, which is unconsciously sacred. Or, perhaps it is not an urge but an accommodation, an implicit accommodation.

I spoke earlier on of moral indecision as the most incurable of mental states perpetrated by the illusion-based sublimation. That was an absolutist judgement, and a static conceptualization. Nature does not allow vacuum, and mind, even the Hindu mind, is part of the jurisdiction of nature. If taking public position is out, private gossip can compensate. And within your family, you may reign supreme about matters moral, giving fullest possible play to suspicion and, where relevant, jealousy. Already, the orbit of 'moral' has shrunk to a thin term of reference. Moral indignation in the widest possible sense has become irrelevant. Beyond me and my family, the society can please itself, leaving me alone to please myself by talking about it all for a sort of self expression in a part of the very area where it is so highly suppressed. As for himself, his own personal self, moral indecision takes a breath-taking manifestation. The Hindu mind does not own his moral wrongs. 'Own', I said; he might beg and cringe, even seemingly invent things against himself. He may indeed chuckle at this sort of his resourcefulness. And for all that I can discern, he might still deserve "forgive him, Lord, for he does not know what he is doing." But, in any case, honest owning...to himself, no.

And the sublimer is also the readily available explainer or, at any rate, Hinduism allows the man to explain things away...the cycle of rebirth, the goal, the law of Karma, all are interpretable in a manner that easily saves one from problems of conscience. Any

acute mortification touching on the moral, tingling with moral mortification, is close to being foreign to the Hindu mental make-up. The Hindu looks around and has no reasons to develop any complexes on this count; they are almost all alike, the highest of the high as well as the lowest of the lowly. Too harsh to be true? Too tenuous? I cannot assert my assertion, but I have a suspicion that I cannot be far wrong. Also, I might have over-stated the attribute. Certainly, the Hindu is not dishonest, any more than any other human being is. He is simply not honest unto himself. Nor is he a greater compromiser. He simply does not recognize his compromises as compromises; how possibly could he if he was nearly not conscious about doing so. And the Hindu no less human than any other is probably more humane than most. He is capable of being aroused and once aroused his emotional outbursts, like any other human's, may be scathing and, more significantly, may ensue in unbelievably coherent action. But, of course, being human, as with all humans, is being animal-like; he can be hurt, enraged, maddened. The provocation may be private, touching on his property or honour, or, if sufficiently motivated, public, touching on his group, community, even nation. There is more of pent up emotion within him than perhaps within anyone else: more of, part dormant, part simmering, pool of energy within him than within anyone else; a volcano which can erupt. He can be revengeful, even scheming. He is also capable of making supreme sacrifice, almost consciously, almost deliberately. And he can be humane, understanding, forgiving, too understanding, too forgiving, perhaps.

I have spoken of his personality turning impersonal. There is another way of looking at it. The Hindu may be said to be too integrated a personality. It is, indeed, the main theme of these reflections that the integration needed to be disintegrated to be capable of being reintegrated, a case of creative destruction; to be loosened up, so that a more flexible, more coherent, more truly systematized personality was released. A total revolution (I am sorry for the contemporary ring), upheaval, is called for. Only then can the Hindu become a consciously as well as vigorously participating agent of his own and his country's march ahead. He may or may not want (need) the release from the cycle of rebirth (should that be the cosmic truth all creatures should want what); but he would certainly appear to want a release from, and unto, himself...a release from moral indecision.

I still do not consider my book to have gone very far astray on the basics. I certainly think that the hypothesis in question was



sound. Only, I now think, it did not go deep enough to stumble on moral indecision. Other-worldliness, as an explanatory variable, would need, if not to be displaced, to be buttressed by moral indecision. I would myself venture the hypothesis that other-worldliness... anywhere... is just one of the manifestations of moral indecision. I realise that in putting the matter like this, I am all but discarding my proposition about the definitive attribute of the Hindu mind. But just as well, and, in any case, not quite. All that it does amount to, and I feel rather relieved that it amounts to this, is that the Hindu, fundamentally, is no different from other human beings; that, in other words, moral indecision is not the preserve of the Hindu. Of course, it is all a matter of emphasis. And I mean the emphasis.

I promised to concentrate on independent India, and I have already consumed so much space. There is no contrariness. I needed time, and space, to develop my new hypothesis. Things should now fall in their places... without my having to consume a lot more space, or time. Let me see.

I shall begin with a question. What is it that distinguishes Gandhi from any in contemporary India? My own answer is, correspondence between precept and practice. An old truth? An anti-climax to the foregoing much ado? In fact, I am asking the correspondence to carry a considerable lot of burden. Moral indecision has only one way of being turned into moral decision... and that way is moral decision itself. The question is, how possibly to bring this about. The answer is, the way Gandhi did it for himself and tried to do it for his countrymen. How did he do it? The answer is, correspondence between precept and practice. I shall explain. But I shall first add the sufficient condition. The correspondence is a destroyer of moral indecision for the person who, in his own behaviour, imbibes it. So, because... and the explanation is simple... the psychological effort needed to achieve the correspondence is at once massive and momentous. Any lapse is a fall from the precipice. The choice has to be firm and rigid. Moral indignation can emerge only in one who has acutely comprehended, and shuddered at, the prospect of the fall from the precipice in his own case. And when it arises, it has already destroyed moral indecision. To be able, however, to cause a spread effect, especially when the term of reference is a whole society, the person imbibing the correspondence must have more or less direct contact with the society in question. In other words, he already must be, or must soon enough come to be, a person of distinctly recognizable eminence. Gandhi, of course, satisfied the sufficient as well as the necessary condition. And, of course,

all those in history who could muster the courage necessary for the correspondence. Incidentally, it follows that people of distinctly recognizable eminence exhibiting divergence between precept and practice engender and strengthen, the opposite of moral decision... moral indecision. I need not elaborate.

Relevance to my universe of discourse—economic growth ? If the urge to live well is, as those who should know emphasize, the most fundamental factor in economic growth, and if moral indecision is, as I have implied, the most fundamental factor inhibiting the urge to live well, the relevance is profound. I don't see how even an individual can (unless, of course, he is already rich) begin to live well, if he does not begin to do something about it, and he cannot do anything about it if he does not have the urge to. And how can he have the urge if he can't make clear choices—decisions? (Naturally I do not expect the absurd retort that he cannot make decisions if he cannot make decisions.) Indeed, moral decision affects economic growth not only through its relation with the urge to live well. It does so through affecting population growth, capital formation, inventiveness, and, as a group, the socio-attitudinal set up as well. I am sure I do not need to elaborate. I can only remind that if a whole people's economic well-being is involved, with all the weight of geography, history, and motivations, the task, and the relevance of moral decision or indecision, is stupendous. There are, of course, both challenges and opportunities.

What has happened in independent India ? One can only say what, in one's own light, has not happened that, again in one's own light, might possibly have happened. I would myself say that the horse has not been induced to drink, that it could have been foreseen that horses cannot be induced to drink, and that, since the horse, in fact, was thirsty, he needed first to be treated rather than quenched, or, at any rate, he needed to be treated as well as quenched. And those who could treat needed themselves to be treated, and the chances are they never knew that anyone needed to be treated. Gandhi knew (he had treated, gone on treating, himself as well as his countrymen), but did not live long in independent India. It would not be polite to mention other names; those that cannot suffer mention are, in any case, small names.

One might be tempted to look up to Russia and China—and force. I would myself suggest that my new hypothesis should curb the temptation. Geographies, and histories, differ; but these can be let pass. The particular manner of sublimation, and the ultimate content of the sublimation, is difficult to be let pass. The Hindu needs a

resublimation which, to begin with, would need to be of the nature of an anti-thesis of the prevailing sublimation.

With Gandhi gone, it is difficult to begin in terms of who could bring this about. One can't order Gandhies, and, despite the Gita's assurance, one can't expect Gandhies to be there on their own when the occasion demands. And, meanwhile, what poor correspondence between precept and practice; the poverty rising almost in direct proportion to eminence, too. But one certainly can begin in terms of how to bring this about. My own answer is already indicated in what I described earlier on as the main theme of these reflections. Did I have China in mind, after all; China, not of the force (the stick) but, of the cultural revolution? No, And so because China of the cultural revolution is no less a China of the force than China without the cultural revolution. As a matter of fact, I would reject the whole conceptualization that exhausts all human motivation and action in terms of the carrot and the stick; and for purely empirical reasons rather than simply (which of course it also is) for being repugnant to human dignity. And I would add that not all force is stick; force can be power, spontaneous, self-propelled power. I have absolutely no doubt that Gandhi was generating such power.

Gandhi is no more. But, surely, his ideas are not dead, or at any rate, they could be made alive. Could I possibly be thinking of his desire to change the individual man, or, perhaps more truthfully, his desire for the individual man to change himself? If I were to say, yes or no, I would say, yes; but, while I simply do not know what exactly, I have the feeling that I should myself want to have something slightly different. My hypothesis does not allow me to go by Gandhi's desire, in the sense that I don't expect it to fructify. As a minimum, it would, for it to fructify, need another Gandhi! And there is no telling whether another Gandhi would have the same desire. But if, in fact, we had another Gandhi he may not have the same desire, but I would expect of him to be engaging in disabusing the Hindu mind of his moral indecision, and I would expect of him to be knowing how to go about it. I know that I am not expecting myself to be another Gandhi, so how could I possibly know how precisely to go about it?

## CHAPTER 17

### The Pattern of Planned Investment in India<sup>1</sup>

It is obvious that, *ceteris paribus*, the higher the rate of investment in an economy, the higher is the rate of growth of national income and, in a two-sector model of, say, the Fel'dman type, given the rate of investment, the larger the investment allocation to the producer goods sector, the higher is the rate of growth of national income—the longer the time horizon (with the corollary to the latter that the larger the investment allocation to the consumer goods sector, the higher is the rate of growth of national income—the shorter the time horizon). Now, given the rate of investment, the ceiling for investment allocation to the producer goods sector (the floor for that to the consumer goods sector) is determined *inter alia* by the initial stock of capital, the rate of depreciation and the capital-output ratio in the two sectors, and the rate of population growth. Keeping a rather loose variant of this as a conceptual framework, the purpose of this paper, which does not aim at being more than a first approximation, is to examine whether, during the period of planning in India, investment in the producer goods sector has been too high relative to that in the consumer goods sector. Section I bring out the pattern of investment on three alternate assumptions. Section II attempts, on not altogether conclusive evidence, to answer the question if investment in the producer goods sector has been too high (or that in the consumer goods sector too low). Section III hazards some remarks on the overall rate of investment, on investment in the consumer goods sector, especially agriculture, and on certain related aspects of the state of the Indian economy generally.

The basic table appended to this paper presents the pattern of investment in India during the three Five Year Plans and that envisaged in the draft Five Year Plan and the notes to the table explain the categorization of investment, the method of calculation

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<sup>1</sup>This paper in collaboration with my colleagues P. C. Jain, R.K. Jain, O.P. Mahajan and Amar Singh, was prepared before the original Fourth Plan had been declared abortive.

etc. Almost all information relating to investment given in the text has been derived from this table and it is to this table and the notes that the reader would need to refer from time to time, especially to be able to appreciate the regrouping of investments on the basis of the three assumptions in this section and a fourth in the following. The assumptions themselves, in effect, mean no more than the particular manner in which investments in a relatively large number of categories are consolidated in a relatively small number of categories, especially in our two analytical sectors, producer goods and consumer goods.

The regrouping does indeed present the main problem. Ideally, one should have liked investments of all kinds to be neatly, *i.e.*, without any conceptual or empirical difficulties whatsoever, divisible between the producer goods and consumer goods sectors. The criterion, one might say, is: that which added to productive capacity of the producer goods sector should go to the producer goods sector, the remaining going to the other, or *vice versa*. This would assume that the twelve categories into which investments have been distributed (Appendix) are themselves amenable neatly to be divisible between the two sectors. In fact, they are not and so, in several cases, we are bound to end up with a certain balance of investments refusing to fall for one or the other sector. Most of these are necessarily of the nature of economic overheads. But there are some others too which are not easily tempted. Take, for instance, the still much-too-inadequate but fairly large investments in major irrigation projects and the poorly investments in fertilizers. Where do they belong? It has to be appreciated that, apart from the long gestation period of such investments, Indian agriculture continues, in terms of the number of producers and, also, not insignificantly, of the proportion of total output, to be overwhelmingly self-consumption biased with the result that the transition from the creation of productive potential to actual input-mix is conditional upon several factors, attitudinal and institutional as well as economic. Actually, as the table and notes in the Appendix show, the twelve categories conceal many more categories and, in some cases, rather illogically. 'Agricultural Programmes', for instance, includes forestry and soil conservation as well as fisheries and animal husbandry. Then there are other irritants, *e.g.*, 'Inventories' having been put in an altogether separate sub-category meant for all large scale industries. And there is the almost inevitable 'Miscellaneous'.

Consequently, we have, first, regrouped all investments which, with certain modifications, coincide with the twelve categories, into

the two sectors, producer goods and consumer goods, on two alternate assumptions—one (Assumption 1) apparently biased towards the producer goods sector and the other (Assumption 2) towards the consumer goods. We have then, on a third assumption (Assumption 3), regrouped all investments into three sectors: producer goods, consumer goods and overheads.

**Assumption 1 :**

Tables I-III below give, in absolute quantities and percentages, the distribution of investment between the producer goods and consumer goods sectors, and the notes following the tables adumbrate the three assumptions involved in the respective regroupings.

**TABLE I**

Distribution of Investment by Sectors in Crores of Rupees and per cent (in brackets) of total investment.

Sectors	I Plan	II Plan	III Plan	Three Plans	IV Plan	Four Plans.
Producer Goods Sector	2358.07 (76.93)	5384.17 (78.88)	9195.43 (80.16)	16937.67 (79.29)	17322.87 (81.26)	34360.54 (80.51)
Consumer Goods Sector	707.17 (23.07)	1441.40 (21.22)	2275.43 (19.84)	4424.09 (20.71)	3894.13 (18.74)	8318.22 (19.49)

Note : The figures in the table are derived from the Appendix, the distribution itself being made on the assumption that the producer goods and consumer goods sectors respectively incorporate the investments in the twelve categories of the Appendix in the following manner.

Producer goods sector :

II. Community development

III. Irrigation

IV. Power

VI. A :

(i) Metals and Metallurgical

(ii) Machinery & Engineering

(iii) Intermediate goods

(iv) Fertilisers and Pesticides

VI. B : Mining

VII. : Transport & Communications

VIII-XII : Social Services (Appendix)

Consumer goods sector :

I. Agricultural Programmes

V. Village & Small Scale Industry.

## VI. A

(v) Consumer goods.

VI A (vi) 'Inventories' and (vii) 'Miscellaneous' have been distributed between the two sectors in the proportion of VI A (i)-(iv) and VI A (v) respectively.

## Assumption 2

TABLE 2

Distribution of Investment by Sectors in Crores of Rupees and percent (in brackets) of total investment.

Sectors	I Plan	II Plan	III Plan	Three Plans	IV Plan	Four Plans.
Producer Sector	1437.08	4025.51	6528.84	11991.43	13530.85	25522.28
Consumer goods sector	1628.16 (53.12)	2800.15 (41.02)	4942.02 (43.08)	9370.33 (43.84)	7786.15 (36.53)	17156.48 (40.20)

Note : The figures in the table are derived from the Appendix, the distribution itself being made on the assumption that the producer goods and consumer goods sectors respectively incorporate the investments in the twelve categories of the Appendix in the following manner :—

## Producer goods sector :

## VI. A

- (i) Metals & Metallurgical
- (ii) Machinery & Engineering
- (iii) Intermediate goods.

## VI. B Mining.

VIII-XII Special Services except X Housing & Construction (Appendix).

## Consumer goods sector :

I. Agricultural Programmes

III. Irrigation

V. Village & Small Scale Industry.

## VI. A

(iv) Fertilisers & Pesticides.

(v) Consumer goods

X. Housing & Construction (Appendix)

VI A (vi) 'Inventories' and (vii) 'Miscellaneous' have been distributed between the two sectors in the proportion of VI A (i)-(ii)-(iii) and VI A (iv)-(v) respectively.

- II. Community Development, IV Power and VII Transport & Communications have been distributed between the two sectors in the proportion of investments in each sector after 'Inventories' and 'Miscellaneous' have been distributed.

### Assumption 3

**TABLE 3**

Distribution of Investment by Sectors in Crores of Rupees and percent (in brackets) of total investment.

Sectors	I Plan	II Plan	III Plan	Three Plan	IV Plan	Four Plans.
Producer Goods Sector	285.54 (9.34)	1691.84 (24.79)	2837.82 (24.74)	4823.90 (22.58)	7103.83 (33.32)	11927.73 (27.95)
Consumer Goods sector	707.17 (23.14)	1441.49 (21.12)	2277.45 (19.84)	4424.09 (20.71)	3894.13 (18.27)	8718.22 (19.49)
Overheads Sector	2063.83 (67.52)	3692.33 (54.09)	6357.61 (55.45)	12113.77 (56.71)	10319.04 (43.41)	24322.88 (52.56)

Note : The figures in the table are derived from the Appendix, the distribution itself being made on the assumption that the producer goods, consumer goods and overheads sectors respectively incorporate the investments in the twelve categories of the Appendix in the following manner :

Producer goods sector :

VI. A

- (i) Metals & Metallurgical
- (ii) Machinery & Engineering
- (iii) Intermediate goods
- (iv) Fertilisers & Pesticides

VI. B Mining

Consumer goods sector :

I. Agricultural Programmes

V. Village & Small Scale Industry

VI. A (v) Consumer goods

Overheads sector :

II. Community development

III. Irrigation

IV. Power

VII. Transport & Communications



## VIII-XII. Social Services (Appendix)

'Inventories' and (vii) 'Miscellaneous' have been distributed between the first two sectors in the proportion of VI A (i)-(iv) and VI A (v) respectively.

The difference in the sectoral distribution of investment between table 1 and table 2 is, of course, due to the assumptions involved. But the difference, which has a definitely rising tendency over the period in table 1 and also, on the whole, in table 2, in favour of the producer goods sector is remarkable enough. Table 3, also because of the assumption involved, carves out by far the major share in investment for the overheads sector. But, as between the producer goods and consumer goods sectors, there is still, except for the first plan which is known to have been essentially non-industrial, a slight difference, with a widening gap, in favour of the former which progressively gained at the cost of the overheads and, to some extent, consumer goods sectors.

Of course, one could derive as varied a picture as one liked depending upon the type and number of assumptions. But then this will be a confusing picture. The picture which we consider to be most reliable is derived from Assumption 1, and our intention in introducing other assumptions has been simply to show a reasonable range of alternatives. Our preference follows from the not very easily disputable criterion that an investment which does not directly and immediately transform itself as part of the input-mix of the consumer goods sector, enabling the sector to release its product into the pipeline, is best treated as a potential and put in the producer goods sector. While one might like to apply the same criterion to the producer goods sector, one should have to remember that, given the constraint of having to operate within a two-sector model, it is more rational to limit the criterion to the consumer goods sector than to do it the other way round.

## II

It is fair to say that the time horizon of Indian Planning is long enough for the proposition, that the larger the investment allocation to the producer goods sector, the higher is the rate of growth of national income, to hold. (Behaviouristically, this implies that the Indian people are assumed to be willing to sacrifice a part of their possible present material gains for those of their children and grand children). This necessarily puts a premium on a larger investment allocation to the producer goods sector. We may now proceed to examine the four determinants of the ceiling for investment allocation to the producer goods sector mentioned at the outset.

The question of initial stock of capital i.e. stock of capital around 1950-51, is materially relevant only to those industries which existed before planning, and these, apart from agriculture, consisted entirely of consumer goods and certain overheads. For planning can be assumed to have ensured the requisite amount of capital stock in the case of newly established, mostly producer goods industries. As to the former, the position regarding excess capacity should indirectly help determine the adequacy or otherwise of the stock of capital. The existence of excess capacity in certain consumer goods industries, e.g., textiles, might give the impression that there was too much investment in such industries has been, if anything, more than adequate. This is actually one of the most misleading aspects of planning in India. There is no question that excess capacity exists but, paradoxically, it has little to do with excessive rate of investment. Indeed, if anything a higher rate of investment in most of these industries, as also in agriculture which provides many of the raw materials, might have helped eliminate such (spurious) excess capacity. One has only to study the state of capital stock in such industries to be convinced of the magnitude of obsolescence there.

The consumer goods sector has also borne the main brunt of the consequences of depreciation; the preceding paragraph may be recalled. That, in Indian conditions, the rate of depreciation in such industries is exceedingly high cannot be disputed. Figures for 1963 indicate that depreciation as a percentage of total productive capital in capital goods, intermediate goods and consumer goods industries amounted to 5.43%, 6.03% and 6.29% respectively and as percentage of fixed capital amounted to 7.12%, 8.97% and 10.92% respectively.<sup>1</sup>

The capital-output ratio is essentially a highly significant factor in this analysis despite its rather mystical character even when generalised for very broad sectors. According to one estimate<sup>2</sup> the ratio, which has invariably been rising, was around the beginning of planning, 2.94, 6.26, 4.76, 2.77 (4.03) and 2.27 in cement, paper, iron and steel, cotton, textiles and sugar respectively. In the new technological horizon of the country, it seems to have been concluded that the ratio must needs be raised. The mostly new producer goods sector need not care a hoot for the simple reason that it has the ratio it must: it is given it by its (latest) technology. There are, however, serious implications in the case of consumer goods industries and agriculture where the question of effective transition from the old to the

<sup>1</sup>Annual Survey of Industries, Govt. of India, 1963.

<sup>2</sup>George Posen, *Industrial Change in India* (1958) p. 75.

new technology creates problems of resources as well as adjustment. On the one hand, the country has been experiencing, despite the patronising attitude towards cottage and such like industries, something like a second phase of ruination, the first having been accomplished by Lancashire; on the other, the so-called small scale industries have turned out to be areas of much higher capital-output ratio than one suspected. All in all, the capital-output ratio in India has, in many cases, been rising for the wrong reasons with unavoidably wrong results. The immediate point, of course, is that a high capital-output ratio requires a high rate of investment, and the consumer goods industries and agriculture would need a rate of investment commensurate with the capital-output ratio of modern technology.

A further set of factors may now be mentioned. First, while the producer goods sector, especially the newly established basic and key industries, have acquired the organisational set up as a necessary and almost obvious concomitant of their techniques, there has unquestionably been, what may be called, an "organisational lag" in the consumer goods sector, the most conspicuous being agriculture. Even when attempts have been made, as in the case of community development projects, panchayats, co-operatives and various agencies for cottage and small scale industries, to cope with the problem, the superstructure has either virtually demolished the base, or remained almost unrelated to the new productive (or social) technology. Secondly, there has been alarming lack of dependable complementary measures and resources and, here again, agriculture has been the major sufferer. Indeed, it is no longer surprising to find that, for instance, major irrigation projects manage somehow to create problems of flood; and flood control measures, of irrigation. Thirdly, there has, beyond doubt, been a slackening of local and traditional efforts and, as in the case of irrigation, old facilities allowed to fall into disuse.

We are on firmer ground in dealing with the fourth determinant, and can right away assert that the prevailing rate of India's population growth (annual average rate being 1.80%, 2.10% and 2.40% for the first three plans respectively) demands a correspondingly higher rate of investment in the consumer goods sector including agriculture than has been given it, especially, if we mean to raise the level of living. The draft Fourth Five Year Plan rather proudly, but quite misleadingly, speaks of per capita availability of food-grains having increased from 12.8 ounces per day in 1950-51 to 15.4 ounces in 1964-65 and of cloth from 11 to 15 metres per annum. But it is well known that, despite heavy imports, there has been a drastic decline

in the following two years in the case of former<sup>1</sup> and, as for the latter, availability, at least because of exports, is less than the figure given.

However, the reader cannot but feel uneasy in being persuaded to rely on the foregoing sketchy and not very well substantiated discussion relating to the four determinants.

**Assumption 4 :** There is, fortunately, a more reliable standard of reference. Norman M. Kaplan<sup>2</sup> has compared the sectoral allocation of investment, particularly, the allocation of industrial investment between basic and non-basic segments of industry, in U.S.A. and USSR during comparable periods of their industrialisation. He defines the heavy or basic segment as industries "which are capital expensive and the products of which are in near universal use as producer's goods." Table 4 below presents, to the extent the Indian data permit, the comparable picture for India.

TABLE 4

Distribution of Investment by Sectors in Crores of Rupees and per cent (in brackets) of total investment.

Sectors	I Plan	II Plan	III Plan	Three Plans	IV Plan	Four Plans.
Industry	690.64 (22.537)	2870.49 (42.05)	5159.86 (44.98)	8720.99 (40.83)	10825.00 (50.79)	19545.99 (45.80)
P.G.S. as % of Industry.	(58.26)	(70.94)	(74.71)	(71.85)	(81.23)	(77.17)
C.G.S. as % of Industry.	(41.74)	(29.06)	(25.83)	(28.15)	(18.77)	(22.83)
Agriculture	959.35 (31.30)	1255.17 (18.29)	2131.00 (18.58)	4345.52 (20.34)	3397.00 (15.95)	7742.52 (18.14)
Transport & Communications	622.20 (20.30)	1410.00 (20.66)	2366.00 (20.63)	4398.20 (20.59)	3640.00 (17.04)	8038.20 (18.83)
Social Services	793.05 (25.87)	1290.00 (18.90)	1814.00 (15.81)	3897.05 (18.24)	3455.00 (16.22)	7352.05 (17.23)

Note : The figures in the table are derived from the Appendix, the distribution itself being made on the assumption that the producer goods and consumer goods sectors respectively incorporate the investments in the twelve categories of the Appendix in the following manner :

<sup>1</sup>Economic Survey 1966-67.

<sup>2</sup>Norman M. Kaplan, Capital Formation and Allocation in Abram Bergson (Ed.) *Soviet Economic Growth* 1953.

## Industry :

(Industry has two sub-categories, PGS, *i.e.* producer goods sector and CGS, *i.e.* consumer goods sector which are assumed to correspond to basic (or heavy) and non-basic in Kaplan's study).

## PGS :

## VI. A

- (i) Metals and Metallurgical
- (ii) Machinery & Engineering
- (iii) Intermediate goods
- (iv) Fertilisers and Pesticides.

## VI. B Mining.

## CGS :

## V. Village &amp; Small Scale Industry.

## VI. A (v) Consumer goods

VI. A (vi) 'Inventories' and (vii) 'Miscellaneous' have been distributed between PGS and CGS in the proportion of VI A (i)-(iv) and VIA (v) respectively, and IV power has been distributed between the two sub-sectors in the proportion of investments in each after 'Inventories' and 'Miscellaneous' have been distributed.

## Agriculture :

- I. Agricultural Programmes
- II. Community Development
- III. Irrigation.

## Transport &amp; Communications :

## VII. Transport and Communications.

## Social Services :

## VIII-XII. Social Services (Appendix)

Now, as against India's investment allocation to the producer goods sector of about 77 per cent of the total industrial investment, Kaplan's corresponding figures for U.S.A. and U.S.S.R., are roughly 32 per cent and 51 per cent respectively. India's figure, based on Assumption 4, might be said to suffer from overestimation. But there is no doubt about India's figure being noticeably higher than that of USSR as well as USA.

It is interesting to note that while, according to Kaplan (same reference), the rate of net investment in USA and USSR was 6%-11% and 12%-15% respectively, that in India was about 6% in the first plan, 11% in the second, 14%-15% in third and (anticipated) 17%-18% in the Fourth plan. The Indian rate thus is decidedly higher

than that of USA and, though still lower than, closer to the Soviet rate. If we now recall the results of Table 4 and join them up with the rate of net investment in the three countries, the presumption that the Indian consumer goods sectors has not received sufficient investment allocation gets further strengthened. The point as : either the rate of net investment in India has been too low or the investment allocation to the producer goods sector too high. It is no wonder that the Indian economy, especially during the second half of the planning period, has been experiencing progressively mounting pressures. Consequently, the question of the floor for investment in the consumer goods sector, especially, agriculture acquires a harrassing, almost harrowing, dimension. One has also to remember that the demonstration effect and the so-called U-sector have been distorting the consumption pattern a little too much, a little too prematurely.

The Indian economy is very likely in for more serious trouble. There are despite, or perhaps because of, a disproportionately high investment allocation to the producer goods sector, no signs yet of any substantial "cost-fall effect", the beginning of a process somewhere in the economy which tends progressively to force costs down. It is not unlikely that sometime in the not too distant future such a process will become visible, but already the damage done by a much too inadequate investment in the consumer goods sector may take long to be corrected. Such a process is accompanied by the emergency of a surplus sector which economies in the process of transformation have almost invariably thrown up, a phenomenon still absent in the Indian economy. This "surplus void" accentuates the pressure economy which India has come to be. A consumer goods surplus sector (textiles in the case of Britain) may no more be feasible for the currently developing economies like India. Nor perhaps is agriculture a very likely sector; and yet agriculture is in some sense a "crucial" sector.<sup>1</sup> An added reason for this is the unemployment aspect of the problem, especially, as the so-called disguised unemployment in the agricultural sector is becoming increasingly open.

To the extent that the growth of national income is a function of the rate of net investment and of the allocation of investment between producer goods and consumer goods sectors, there must be something wrong with the rate of investment or the allocation or both. There is still the possibility that time will justify the Indian allocation. But one has to be a little surer in planning. The rate of

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<sup>1</sup>Vikas Mishra : The Growth Multiplier, Asia Publishing House.

net investment, in one sense, appears to be entering the picture in a rather passive manner, viz., it has not been high enough to leave enough (in absolute terms) for the consumer goods sector.

It would be a travesty of our position if it was concluded that we necessarily advocate a slowing down of the rate of investment in the producer goods sector. While we cannot, and do not, want to escape giving the impression that, given the rate of net investment in the economy as a whole, the investment in the producer goods sector has been at the cost of the consumer goods sector, we think that there is, for the future, no room for a pessimism which would suggest an inevitable lowering of the rate of investment in the producer goods sector. It does seem, however, that before the Fourth Plan confirmed its draft proposals for investment allocation to the two sectors, the economy must be assured of an adequate increase in the rate of net investment in the economy as a whole. Else the allocation must be lowered for the former and raised for the latter.

The decision to increase the rate of net investment in the economy as a whole, which would be necessary to maintain the existing percentage allocation of investment to the producer goods sector, necessarily calls for determined political action. The other, easy, alternative is, of course, to reduce the percentage allocation of investment to the producer goods sector.